THE ACCOUNTING REVIEW

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FORECASTS TO ANALYSTS' RECOMMENDATIONS

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EDITORIAL

Introduction to a Forum on the Mapping from Analysts' Forecasts to Analysts' Recommendations

Steven J. Kachelmeier
The University of Texas at Austin

The Accounting Review received the first versions of Barniv et al. (2009) and Chen and Chen (2009) ten days apart in February 2008. Different reviewers evaluated the two manuscripts and returned mostly favorable reports, with no reviewer identifying the other work as a known contemporaneous study. However, as the reader will see (and as is very well explained in Mark Bradshaw's excellent synthesis that closes this forum), the two studies exhibit considerable overlap. Both studies use similar methods to address similar research questions involving the impact of recent regulatory reforms on the linkage from analysts' forecasts to analysts' recommendations, obtaining similar findings and reaching similar conclusions.

If not for the coincidence of timing, most cases of this nature result in reviewers questioning the incremental contribution of the later study over the earlier study. As Bradshaw (2009) notes, even if a second study of the same question is lucky enough to get published somewhere, it typically receives far less acclaim (see Bradshaw's footnote 2 for an interesting analogy to popular music). As Senior Editor of *The Accounting Review*, I see merit to an incremental contribution standard. Journals face capacity constraints, such that I tend to agree with Maines et al. (2006, 85) that the primary criterion on which research should be evaluated is the extent to which it offers "consequential belief revision," either to bolster or to refute our "priors." However, subjecting either of the two research articles in this forum to a strict "incremental contribution" standard over the other article would have been arbitrary and unfair, as there was no practical or substantive basis for determining which study came first, and the reviewers of both studies were largely supportive of publication for similar reasons.

I therefore decided to accept both studies for publication in this forum. The fact that two research teams independently tested the same question and reached similar conclusions using similar empirical specifications adds credence to the validity of their conclusions (Dewald et al. 1986). I am also hopeful that this forum can serve as an educational vehicle for doctoral students, who rarely have such a clean opportunity to compare and contrast two different approaches to the same basic research objective. To that end, I am tremendously grateful to Mark Bradshaw for agreeing to write the commentary that closes this forum. Bradshaw's (2004) study in this journal motivated both Barniv et al. (2009) and

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I am grateful to the reviewers of both Barniv et al. (2009) and Chen and Chen (2009), and I am especially grateful to Mark Bradshaw for agreeing to synthesize insights from both studies in an invited commentary that closes this forum. Professor Bradshaw also kindly provided valued feedback on this editorial.

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Chen and Chen (2009), and I am confident that Professor Bradshaw's current comments will be of much value to those who are contemplating future work in the area.

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Do Analysts Practice What They Preach and Should Investors Listen? Effects of Recent Regulations

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ABSTRACT: From 1994 to 1998, Bradshaw (2004) finds that analysts' stock recommendations relate *negatively* to residual income valuation estimates (scaled by current price) but *positively* to valuation heuristics based on the price-to-earnings-to-growth ratio and long-term growth. These results are surprising, especially considering that future returns relate positively to residual income valuation estimates and negatively to heuristics. Using a large sample of analysts for the 1993–2005 period, we consider whether recent regulatory reforms affect this apparent inconsistent analyst behavior. Consistent with the intent of these reforms, we find that the negative relation between analysts' stock recommendations and residual income valuations is diminishing following regulations. We also show that residual income valuations, developed using analysts' earnings forecasts, relate more positively with future returns. However, we document that stock recommendations continue to relate negatively with future returns. We conclude that recent regulations have affected analysts' outputs—forecasted earnings and stock recommendations—but investors should be aware that factors other than identifying mispriced stocks continue to influence how analysts recommend stocks.

We are very grateful for valuable comments received from two anonymous reviewers, Steven Kachelmeier (senior editor), and workshop participants at Baylor University, the Norwegian School of Economics and Business Administration, and Texas Tech University. Professor Barniv acknowledges the award and financial support of the Division of Research and Graduate Studies at Kent State University, and Professor Hope acknowledges the financial support of the Social Sciences and Humanities Research Council of Canada and the Deloitte Professorship.

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I. INTRODUCTION

Using an extensive sample of sell-side financial analysts, we first examine how Regulation Fair Disclosure (Reg FD) and other recent regulatory reforms (e.g., NASD Rule 2711, NYSE Rule 472, and the Global Research Analysts Settlement) affect the relation between analysts' stock recommendations and (1) theoretically derived residual income models versus (2) valuation heuristics based on the price-to-earnings to growth (PEG) ratios and long-term growth (LTG) forecasts. Our second set of tests involves one-year-ahead excess stock returns. We examine the impact of regulations on relation between future returns and (1) stock recommendations, (2) residual income models, and (3) valuation heuristics. Finally, we consider the extent to which residual income models and valuation heuristics are incremental to stock recommendations in explaining future returns after regulations are implemented.

This research is important because it speaks directly to an issue of great interest to investors and regulators: To what extent do regulations impact financial information provided by an important group (i.e., financial analysts)? Given the widespread availability of financial analysts' earnings forecasts and stock recommendations, our results have practical importance to the investment community and regulators, as well as implications for academic research. While our first set of tests provides understanding of how analysts incorporate their own earnings forecasts into their stock recommendations, our tests of future returns have direct importance to investors. Furthermore, given the historical problems associated with stock recommendations, the extent to which valuation estimates (based on analysts' earnings forecasts) provide explanatory power beyond stock recommendations for future returns will be particularly important to investors.[1]

Presumably, analysts use their own publicly issued earnings forecasts to derive intrinsic value estimates. In this case, one should expect these estimates to relate to analysts' stock recommendations (e.g., Schipper 1991). When earnings-based intrinsic value estimates are above (below) the current stock price, analysts would issue a buy (sell) recommendation. If instead, analysts' recommendations are based on other factors (beyond sophisticated earnings-based valuation estimates), then valuation estimates may provide incremental explanatory power beyond recommendations for future stock performance.

In an interesting recent study, Bradshaw (2004) uses a sample of U.S. firms from 1994 to 1998 and finds that residual income valuations, developed using analysts' earnings forecasts, do not relate as expected to analysts' recommendations. Analysts give more favorable recommendations to stocks with lower residual income valuations relative to current price.[2] Instead, analysts' recommendations align more closely with their LTG forecasts and the PEG ratio. These findings suggest that analysts give the highest recommendations to growth stocks, and among growth stocks, they give the highest recommendations to the firms for which the value of growth estimated by the PEG model exceeds the current stock price. Bradshaw (2004) concludes that analysts rely on simple heuristics rather than more sophisticated residual income valuations to recommend stocks.[3]

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[1] We do not suggest that all investors use both analysts' earnings forecasts and stock recommendations when making investment decisions. Sophisticated investors may use analysts' earnings forecasts and ignore their stock recommendations. Unsophisticated investors may be more likely to rely on analysts' stock recommendations; which require minimal analytical processing. As an example, Bonner et al. (2003) find that sophisticated investors have greater knowledge of the analyst- and forecast-specific factors that predict forecast accuracy, and they use these factors to predict the relative accuracy of analysts' forecast revisions.

[2] In certain specifications, Bradshaw (2004) finds no relation between residual income valuations and stock recommendations.

Bradshaw (2004) also finds that residual income valuations, developed using analysts' earnings forecasts, relate *positively* to future excess stock returns. In other words, analysts' earnings forecasts are useful inputs into residual income valuation models, yet they tend to relate negatively or insignificantly to analysts' stock recommendations. Furthermore, LTG forecasts, which most closely align with analysts recommendations, relate *negatively* to future returns. It seems that analysts recommend stocks with strong growth potential, even if such potential is already impounded into the stock price. Consistent with these results, Bradshaw (2004) shows that stock recommendations are not significantly associated with buy-and-hold one-year future returns.[4] Recommendations do not appear to capture stocks' intrinsic values relative to their current prices.

Why do analysts appear to avoid using their valuable earnings forecasts in a sophisticated manner in setting their recommendations (i.e., fail to practice what they preach)? This surprising result makes this area of research interesting and motivates further examination of the link between valuation estimates and recommendations, and their relations to future stock returns. It could be that analysts have incentives other than using their recommendations to signal mispriced stocks. In fact, analyst behavior has received widespread criticism in the financial press and several groups have called for reforms to the analyst industry.[5] We examine how recent regulations (e.g., Reg FD, NASD Rule 2711, NYSE Rule 472, and the Global Research Analysts Settlement) affect the way valuation estimates map into recommendations and subsequently relate to future stock returns. Specifically, we test for differences in these relations between the 1993–1999 and 2000–2005 periods to determine the impact of Reg FD. Then, we test for differences between the 2000–2002 and 2003–2005 periods to test for effects of other regulations.

Our results show that several important relations change across the regulation periods, while some interesting relations seem unaffected by the regulations. Prior to Reg FD, we find results generally consistent with Bradshaw (2004), even though our sample is substantially larger than his. Following Reg FD, we show that the negative relation between recommendations and residual income valuations becomes significantly smaller and even turns positive for one of our models. However, this change appears to be attributable primarily to regulations other than Reg FD. LTG forecasts continue to have a positive relation with recommendations in the post-Reg FD period, but the relation is weaker. PEG valuations have an increasingly positive relation with stock recommendations over our regulatory period.

In our next set of tests, we examine how valuations and recommendations relate to future stock returns. Like Bradshaw (2004), we find that residual income valuations relate positively to future returns. This relation becomes more positive following Reg FD. Furthermore, the increasing positive relation appears attributable to Reg FD as we find no evidence of an impact of other regulations. We find that the relation between LTG forecasts and future stock returns is significantly negative in the pre-Reg FD period and immediately following Reg FD. After regulations subsequent to Reg FD, LTG and future stock returns become slightly less negatively related. Finally, and perhaps of greatest interest to investors, stock recommendations have a significantly *negative* relation with future stock returns. Even though analysts' earnings forecasts are useful (in residual income valuation models) for predicting stock performance, their recommendations seem to predict the opposite performance. We find that the negative relation between recommendations and future stock performance persists after Reg FD but subsequent regulations have significantly reduced this negative relation. Overall, we conclude that regulatory reforms seem to be adjusting analysts' outputs (i.e., earnings forecasts and stock recommendations) in the expected direction, but the adjustment may be incomplete. Reg FD has played a greater role in increasing the usefulness of earnings forecasts, whereas regulations subsequent to Reg FD have had a greater effect on stock recommendations.

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[3] These results are consistent with those in Gleason et al. (2007) who conclude that analysts rely on simple heuristics rather than formal valuation models in setting price targets. Bradshaw and Brown (2005) conclude that analysts face greater incentives to provide accurate earnings forecasts than target prices.

[4] Other recent studies find mixed results on the usefulness of stock recommendations (Womack 1996; Barber et al. 2001, 2003; Mikhail et al. 2004; Li 2005; Gleason et al. 2007).

[5] Boni and Womack (2002) provide a useful overview of these issues and list many references to both practitioner and research articles.

In the next section we summarize the related literature and discuss our framework for analyzing the analyst/investor relation, highlight objectives of recent regulations (and discuss some research findings related to these regulations), and present our hypotheses. In Section III we briefly describe the valuation models, and in Section IV we discuss our sample selection and descriptive statistics. Section V provides our main empirical findings as well as results from additional analyses. Section VI concludes.

II. PRIOR RESEARCH AND HYPOTHESES

In this section, we first describe the framework in which we analyze the analyst/investor relation. Then we focus on identifying factors that can affect this relation when examining analysts before and after recent regulatory reforms. Finally, we present our hypotheses.

Analyst/Investor Relation

Schipper (1991) encourages research to help better understand how earnings forecasts relate to stock recommendations. She argues that forecasts should be viewed as an input into producing a final output (i.e., a recommendation) and not just a stand-alone final output. We expect the following relations between analysts and investors. First, analysts gather firm-specific, industry-specific, and economy-wide information to generate earnings forecasts. Next, analysts input these earnings forecasts into a valuation model to compute an intrinsic value of the firm. Then, analysts issue recommendations based on comparing estimates from these valuation models with current stock prices. When the model indicates an intrinsic value above (below) the current price, analysts will issue a buy (sell) recommendation. Investors then adjust prices for the analyst's recommendation. If the academic research correctly identifies the analyst's *unobservable* valuation model, then a positive relation between valuation estimates (scaled by current price) and *observable* stock recommendations is expected.

Bradshaw (2004) examines whether valuation estimates based on analysts' earnings forecasts are consistent with their stock recommendations. He considers two residual income models, the PEG model, and LTG forecasts.[6] All valuation estimates rely on analysts' earnings forecasts. Perhaps surprisingly, he finds that residual income valuations are either unrelated to or *negatively* related to recommendations. But, these valuations are *positively* associated with future stock performance.[7] In addition, he finds that recommendations are

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[6] Details on these four models appear in Section III.

[7] Frankel and Lee (1998) also find a positive relation between residual income valuations and future stock performance.

unrelated to future stock performance.[8] From this evidence, one concludes that analysts' earnings forecasts provide useful information to investors for predicting future stock performance but analysts' recommendations do not. In other words, analysts do not appear to practice (recommend) what they preach (forecast). Our primary objective is to investigate the effects of recent regulations affecting analysts' work environments on the above relations.

Mitigating Factors

Several factors provide possible explanations for Bradshaw's (2004) surprising results. For example, after issuing an earnings forecast, the analyst might not employ rigorous valuation models but instead rely on simple heuristics, whereas investors rely on more sophisticated residual income models. Bradshaw (2004) finds evidence consistent with LTG forecasts being the most important determinant of stock recommendations, regardless of the degree to which these expectations are already impounded in stock prices. These results suggest that analysts tend to rely on valuation heuristics to a greater extent than on more "theoretically driven" residual income models. These archival results are consistent with findings in broad surveys of analysts (e.g., Barker 1999; Block 1999) as well as detailed analyses of small samples of research reports (e.g., Bradshaw 2002). Bradshaw (2002) examines 103 U.S. analyst reports and finds that analysts frequently support their stock recommendations with a PEG model. Asquith et al. (2005) investigate *Institutional Investor* "All American" analysts, presumably the most sophisticated analysts, and find that only 13 percent of their reports refer to discounted cash flows in formulating price targets. Results in Gleason et al. (2007) are also consistent with analysts' use of simple heuristics rather than more rigorous residual income models.

In addition, in setting their recommendations, analysts may consider factors other than the intrinsic value estimates relative to current stock prices. Rather than maximizing gains to investors, analysts may be serving personal objectives, such as increasing their compensation, improving relations with management, garnering investment banking business for the brokerage firm, "hyping" the stock to garner brokerage trading volumes, and increasing the value of shares personally owned (e.g., Lin and McNichols 1998; Michaely and Womack 1999, 2005; Ertimur et al. 2007; Ke and Yu 2007). For example, Gimein (2002) claims that investment advice offered by analysts is "so dishonest and fraught with conflicts of interest that it has become worthless" (see also Heflin et al. 2003). As evidence of this, prior research demonstrates that affiliated analysts (i.e., those having direct investment banking business with the firm) issue more optimistic forecasts (Dugar and Nathan 1995; Lin and McNichols 1998; Dechow et al. 2000). Das et al. (1998) and Lim (2001) suggest that forecast optimism is used to increase access to management, especially in cases where the information asymmetry between management and investors is high.[9]

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[8] Womack (1996) and Barber et al. (2001) find that recommendation changes are associated with future stock returns. Other recent studies find mixed results on the usefulness of stock recommendations (Barber et al. 2003; Mikhail et al. 2004; Li 2005; Gleason et al. 2007). The combined evidence suggests that analysts' earnings forecasts provide useful information for measuring intrinsic values but that analysts' recommendations do not. Barber et al. (2006) suggest that market prices react slowly to the information contained in recommendations.

[9] Francis et al. (2004) provide an in-depth review of the evidence on security analyst independence and conclude that there is strong evidence that U.S. analysts behave in a biased manner. Using the tests in Bradshaw (2004), Barniv et al. (2008) investigate strong investor protection versus weak investor protection countries and conclude that analyst bias is more pervasive in strong investor protection countries. This result is consistent with analysts' stock recommendations in strong investor protection countries being affected more by factors other than identifying mispriced stocks.

If stock recommendations are set based on incentives other than (only) identifying mispriced stocks, then the relation between stock recommendations and future stock performance is expected to be low or even negative. This may further explain why Bradshaw (2004) finds no significant relation between the level of analyst recommendations and future annual excess returns during his 1994–1998 sample period.[10] These alternative motivations are certainly consistent with the well-documented optimistic bias in analysts' stock recommendations.[11]

Regulatory Reforms

In recent years several important developments in the regulatory environment have affected sell-side financial analysts, and these reforms have the potential to significantly change analysts' incentives or behavior and therefore their output (e.g., earnings forecasts and stock recommendations). Our study tests whether relations between recommendations and valuation estimates are affected by changes in the regulatory environment over time and thus sheds light on whether potential changes in the relations are consistent with the objectives of the reforms.

Reg FD, issued by the Securities and Exchange Commission (SEC) in October 2000, prohibits firms from selectively disclosing management information to analysts. The purpose of the reform was to level the playing field by giving all equal access to material information released by management. Some contend that prior to Reg FD, analysts would purposely bias their earnings forecasts to gain favor with management, thereby allowing easier access to inside information or investment banking business. If Reg FD eliminates the ability to gain privileged information, then one motivation for providing purposely biased earnings forecasts has been eliminated, presumably leading to improved usefulness of earnings forecasts.

Herrmann et al. (2008) find evidence to support this notion.[12] They conclude that Reg FD reduces the incentive for analysts to provide optimistically biased forecasts of internationally diversified firms, potentially improving the quality of analyst forecasts and the decisions of investors based on those forecasts. Others may argue that Reg FD has not led to improved earnings forecasts. Some research suggests that forecast accuracy decreases and forecast dispersion increases following Reg FD (e.g., Bailey et al. 2003; Agrawal et al. 2006). Based on their findings, Agrawal et al. (2006) conclude that a reduction has occurred in both selective guidance and the quality of analyst forecasts after Reg FD. Thus, although the intent of Reg FD is clear and should indicate a strengthened association between analysts' earnings forecasts and their stock recommendations, there is mixed empirical evidence regarding the possible effects of Reg FD on analysts' work environment and their earnings forecasts.

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[10] Jegadeesh et al. (2004) find that recommendation levels are positively related to subsequent returns only for firms with favorable quantitative characteristics such as value stocks and positive momentum stocks. Womack (1996) and Barber et al. (2001) examine changes in analysts' recommendations and conclude that these are positively associated with future excess returns. In this paper, we choose to follow Bradshaw (2004) and Jegadeesh et al. (2004) and examine recommendation levels. First, we want to be able to compare our results with those in Bradshaw (2004). Second, we want to examine recommendations the way an investor who does not rely on computer-generated trading would process recommendations. Such an investor would find a stock, check out the outstanding recommendations, and then buy/not buy/sell.

[11] For example, Jegadeesh et al. (2004) report that approximately 80 percent of the recommendations are Buy or Strong Buy, and only 5 percent are Sell or Strong Sell.

[12] Using the extent of a multinational firm's international operations to proxy for analysts' need to gather privileged information from management, Herrmann et al. (2008) show that the relation between forecast bias (optimism) and international diversification significantly declines (and even disappears) in the post-Reg FD period.

In addition to Reg FD, other recent regulatory reforms also potentially impact the output of financial analysts. Because of huge investor losses as a result of the crash of technology stocks between 2000 and 2002, regulators came under pressure to "fix" analysts' research reports. It was analysts' overly optimistic research reports that were often cited as a key factor leading to the run up of security prices in the late 1990s. For example, by the end of 1999, less than 1 percent of analysts provided "sell" recommendations (Bogle 2002). The investing public argued that analysts employed by brokerage firms that offered both investment banking business and research reports faced a conflict of interest. The conflict arose because in an attempt to maintain investment banking business for the brokerage firm, analysts faced pressure to provide favorable research reports (i.e., buy recommendations) instead of providing objective research to the investment community. As a result of these criticisms, regulators proposed NASD Rule 2711 (*Research Analysts and Research Reports*) and an amendment to NYSE Rule 472 (*Communications with the Public*) in 2002. In general, the proposed regulatory changes were directed at limiting interactions and flow of information between analysts who provide recommendation reports and the investment banking business of the brokerage firm.[13] These proposals were formally accepted by the SEC on July 29, 2003.[14]

In December 2002, the SEC announced the Global Research Analyst Settlement, which was enforced in April 2003 (SEC 2002). Here, the SEC reached a legal settlement with the New York Attorney General, NASD, NYSE, state regulators, and ten of the top U.S. investment firms. The settlement describes how analysts from leading banks provided misleading information to investors, allegedly because of investment banking incentives.[15] In particular, the settlement discloses that analysts issued positive public information that conflicted with their negative views about the stock (De Franco et al. 2007). In other words, as discussed above, investment banking incentives can lead to misleading analyst behavior.[16]

There is some evidence that these regulations have impacted analysts' recommendations. Kadan et al. (2006) show that prior to these regulations, analysts were 40 percent more likely to issue an optimistic recommendation for stocks that had recently undergone an initial public offering or seasoned equity offering. This probability increased by an additional 12 percent when the recommendation was made by an affiliated analyst. These effects vanished after regulations. Barber et al. (2006) support this notion by documenting a decrease in the overall percentage of buys in broker ratings between January 2000 and June 2003, particularly among sanctioned investment banks. Consistent with these findings,

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[13] For a complete description of the rules see http://www.nyse.com/pdfs/rule472.pdf for NYSE Rule 472 (2002) and http://finra.complinet.com/finra/display/display.html?rbid=1189&element_id=1159000466 for NASD Rule 2711 (2002).

[14] Rule 2711 covers restrictions on relationships between the investment banking and research departments, restrictions on review of a research report by the subject company, prohibition of certain forms of research analyst compensation, prohibition of promise of favorable research, restrictions on personal trading by research analysts, and disclosure requirements. This rule was introduced on May 10, 2002, but its implementation was subsequently delayed several times (SEC 2002). It seems likely that the mere "threat" of its implementation could have an effect on analyst behavior.

[15] The settlement also requires the brokerage firms to make structural changes in the production and dissemination of analyst research.

[16] The SEC further issued several releases governing investment firms' disclosure practices in 2003 (e.g., Regulation Analyst Certification [AC] 2003). Regulation AC requires certifications by analysts that the views expressed in their research reports accurately reflect their personal views. Analysts are required to disclose whether they receive any direct or indirect compensation for their reports. Analysts who cannot certify that they have not received compensation for a specific report must disclose the magnitude and source of the compensation. Finally, the Sarbanes-Oxley Act came into effect in 2002, potentially affecting the quality of financial reporting and thus the work of financial analysts.

Ertimur et al. (2007) and Ke and Yu (2007) show that the improvement is analysts' recommendations around recent regulations was greater for analysts that likely faced higher conflicts of interest.[17]

In summary, recent regulations have addressed bias in analysts' earnings forecasts and stock recommendations. If these regulations have had their intended effects, then we should observe an increase in the usefulness of analysts' output—earnings forecasts and stock recommendations. This leads us to the following set of hypotheses.

H1: Following recent regulations, the relation between analysts' stock recommendations and earnings forecast-based residual income (heuristic) valuations is expected to become more (less) positive.

H2: Following recent regulations, the relation between earnings forecast-based residual income valuations and future stock returns is expected to become more positive.

H3: Following recent regulations, the relation between analysts' stock recommendations and future stock returns is expected to become more positive.

III. A BRIEF DESCRIPTION OF VALUATION MODELS

In this section, we briefly describe the valuation models used in this paper.[18] Following prior literature (e.g., Ohlson 1995; Frankel and Lee 1998; Bradshaw 2004), we estimate the residual income model as the present value of expected residual income for the next five years plus a terminal value:

$$V_t = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1+r)^{\tau}} + \frac{E_t[TV_{t+5}]}{(1+r)^{5}}. \tag{1}$$

To estimate Equation (1), we require availability of book value per share (*BVPS*) in year *t* from Compustat and forecasted earnings per share for years $t+1$ and $t+2$ from I/B/E/S. If available, we use analysts' forecasts of years $t+3$ through $t+5$. If not available, we extrapolate earnings forecasts for these years using the earnings forecast for year $t+2$ and the long-term growth forecast.[19] Residual income (*RI*) equals forecasted earnings, less the discount rate (*r*) times the prior year's book value. Future book values are extrapolated from book value in year *t* using the clean surplus assumption (i.e., $BVPS_{t+1} = BVPS_t + EPS_{t+1} - DPS_{t+1}$), where future earnings, EPS_{t+1}, are forecasted earnings, and future dividends, DPS_{t+1}, are measured using the assumption of a constant payout ratio based on year *t*.

Due to the importance of assumptions embedded in the terminal value (*TV*) computation, we estimate two versions of the residual income model (Bradshaw 2004). The first, V_{RI1}, assumes that abnormal profits are driven away over time due to competitive pressures. In practice we build in a fade rate (ω) that implies that residual income reverts to zero over ten years:

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[17] Specifically, Ke and Yu (2007) provide an interesting study of how analyst ability, analyst independence, and investor sentiment affect the efficiency with which analysts incorporate their own earnings forecasts into stock recommendations around recent regulations.

[18] For more on these models, see Frankel and Lee (1998), Lee et al. (1999), Liu et al. (2002), Easton (2004), and Hope et al. (2009).

[19] For example, if forecasted earnings for year $t+2$ equal \$1.00 and the long-term growth forecast is 10 percent, then forecasted earnings for year $t+3$ is \$1.10, forecasted earnings for year $t+4$ is \$1.21, and forecasted earnings for year $t+5$ is \$1.33. To provide this extrapolation, we require that forecasted earnings for year $t+2$ be positive.

$$V_{RI1,t} = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1+r)^{\tau}} + \frac{\omega E_t[RI_{t+5}]}{(1+r-\omega)(1+r)^5}. \tag{2}$$

The second specification of the residual income valuation model (V_{RI2}) assumes that residual income in the terminal year persists in perpetuity, which is a more optimistic assumption than the fade-rate assumption used for V_{RI1}:

$$V_{RI2,t} = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1+r)^{\tau}} + \frac{E_t[RI_{t+5}]}{r(1+r)^5}. \tag{3}$$

Barker (1999), Block (1999), Bradshaw (2002), and Chen et al. (2004) discuss how analysts use price-earnings-based techniques in practice. Numerous articles in the financial press describe the pervasiveness of the use of the "PEG ratio" as a basis for stock recommendations. For example, Peter Lynch advocates the PEG ratio in his book *One Up on Wall Street* (Lynch 2000). The *PEG* ratio is defined as:

$$PEG_t = \frac{P_t / E_t[EPS_{t+2}]}{LTG_t * 100}, \tag{4}$$

where P is stock price, $E_t[EPS_{t+2}]$ is forecasted earnings per share in year $t+2$, and LTG is the long-term growth forecast. Following Bradshaw (2004), we compute the *PEG* valuation as:

$$V_{PEG,t} = E_t[EPS_{t+2}] * LTG_t * 100. \tag{5}$$

V_{RI1}, V_{RI2}, and V_{PEG} are divided by current stock price. To the extent that the valuation estimate is greater (less) than current price, the valuation model suggests an under (over) priced stock and therefore higher (lower) future returns, on average.

Finally, although not a valuation estimate per se, we include *LTG* forecasts as our fourth metric. This is important since *LTG* forecasts seem to be the primary measure used by analysts in setting their recommendations prior to regulations (Bradshaw 2004), yet they have a strong *negative* relation with future stock returns. We are interested in the impact that recent regulations have on the use of heuristics by analysts. While an increase in the relation between residual income valuations and stock recommendations might provide indirect evidence of a reduced reliance on heuristics, this is not necessarily the case. We believe it is important to provide a direct test. Providing results for each of these contrasting relations (heuristics versus theoretically driven residual income values) provides additional evidence for understanding the link between analysts' earnings forecasts and their recommendations.

IV. DATA, SAMPLE, AND DESCRIPTIVE STATISTICS

We obtain data on annual consensus earnings forecasts, projections of long-term earnings growth, and stock recommendations from I/B/E/S for the sample period January 1993–May 2005 for an extensive sample of firms.[20] Our initial sample includes 425,158 observations that have stock recommendations and data necessary to create our four valuation estimates.[21] Next, we exclude observations for months without changes in stock recommendations.[22] Since recommendations can be fairly sticky across months, using only months that involve a change in recommendations provides a more realistic setting of when analysts are more likely to incorporate current information into their recommendations (as opposed to current recommendations reflecting stale information). The final sample consists of 187,889 monthly observations representing 8,079 firms. We have 112,477 observations for our pre-Reg FD (1993–1999) sample and 75,412 observations for our post-Reg FD (2000–2005) sample. Note that our pre-Reg FD sample is substantially larger than the one employed by Bradshaw (2004) of 15,318 observations over the 1994–1998 period (with LTG available, which we require for all of our tests).[23] Within the post-Reg FD sample, we have 36,799 observations prior to other regulations (2000–2002) and 38,613 observations for 2003–2005 (after other regulations). We refer to the periods before and after other regulations as the pre-OtherReg and post-OtherReg periods.

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[20] Bradshaw (2004) uses First Call as his source for analyst data. First Call and I/B/E/S differ in that First Call includes consensus data for a month only if the consensus was revised during the month. I/B/E/S is more comprehensive in that it includes all months, including those with no changes in the consensus. We base our main results on using change months only (consistent with Bradshaw [2004]), but we show later in the paper that results are robust to using the full sample of observations.

Panel A of Table 1 presents descriptive statistics for the pre- and post-Reg FD periods. Consistent with our prediction that Reg FD should reduce analyst optimism, the mean recommendation (*REC*) is significantly lower (at the 1 percent level) in the post-Reg FD era (3.72) than in the pre-Reg FD era (3.96) (1 = Strong Sell to 5 = Strong Buy). The percentage of buy and strong buy recommendation decreases from 67.7 to 47.1, and the percentage of sell and strong sell recommendations increases from 1.1 to 4.4 percent. The means of V_{RI1}/P and V_{RI2}/P significantly increase and V_{PEG}/P and *LTG* significantly decrease.[24] As expected, firm size (market value of equity) increases. In addition, the number of analysts per firm also increases.

Consistent with their high recommendation levels, analysts estimate high long-term growth rates (*LTG*) for the companies they follow: 18.9 percent and 18.0 percent for the pre- and post-Reg FD periods, respectively (and the difference is significant at the 1 percent level). In untabulated analyses, we find that the mean actual annual earnings growth is 8.4 percent and 11.5 percent in these periods. These findings suggest that *LTG* projections are high and optimistically biased, but that this optimism has decreased somewhat in the post-Reg FD period.

Panel B of Table 1 presents the results for the pre-OtherReg period (2000–2002) and post-OtherReg period (2003–2005). The mean recommendation continues to significantly decline, going from 3.89 to 3.58.[25] The percentage of buy and strong buy recommendations

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[21] Results are similar if we relax the requirement that *LTG* forecasts be available (and thus have larger sample sizes).

[22] As a sensitivity test near the end of the paper, we discuss results when all months are included. All conclusions are unaffected. In addition, we have estimated all models after excluding consensus recommendations based on just one recommendation and the results are similar to those reported.

[23] As discussed below, we find results similar to Bradshaw (2004) for the pre-Reg FD period with a few exceptions.

[24] The fact that the mean recommendation *REC* is a buy and the mean residual income valuation estimates (V_{RI1}/P and V_{RI2}/P) are less than 1 suggests that analysts rely on more than just these valuations when deciding their stock recommendations (Bradshaw 2004). Unlike the residual income valuations, the *PEG* valuation is greater than the current price for the pre-Reg FD period (1.14) but is below current price for the post Reg FD (0.79).

[25] One potential alternative reason for the decline in recommendation levels over our sample period could be deteriorating economic conditions. We cannot exclude this possibility. However, it should be noted that recommendations are generally made with the explicit understanding that they represent whether a stock will underperform or outperform the market in general, and not necessarily whether the stock price is expected to decrease or increase. Thus, it is not necessarily the case that poorer economic conditions would lead to reduced recommendations in general.

TABLE 1
Descriptive Statistics

Panel A: Descriptive Statistics for Pre- and Post-Reg FD Periods

	Pre Reg-FD (1993–1999) n = 112,477			Post Reg-FD (2000–2005) n = 75,412			Difference	
Variable	**Mean**	**Median**	**SD**	**Mean**	**Median**	**SD**	**t-test**	**Wilcoxon Z**
REC	3.96	4.00	0.53	3.72	3.75	0.54	−92.5***	−89.7***
%Buy	67.7%			47.1%				
%Sell	1.1%			4.4%				
V_{RI1}/P	0.63	0.58	0.37	0.66	0.62	0.43	19.0***	24.2***
V_{RI2}/P	0.70	0.66	0.42	0.77	0.74	0.53	32.1***	45.0***
V_{PEG}/P	1.14	1.06	1.03	0.79	0.85	1.23	−65.7***	−81.0***
LTG	18.85	16.07	10.47	18.01	15.17	10.22	−17.4***	−20.8***
SAR	−0.027	−0.092	0.598	−0.038	−0.090	0.514	−3.41***	1.62
MV	5,127	821	18,215	7,471	1,249	24,248	22.6***	51.7***
NUM	9.42	7.00	7.02	10.56	9.00	7.13	34.2***	41.2***

Panel B: Descriptive Statistics for Pre- and Post-OtherReg Periods

	Pre-OtherReg (2000–2002) n = 36,799			Post-OtherReg (2003–2005) n = 38,613			Difference	
Variable	**Mean**	**Median**	**SD**	**Mean**	**Median**	**SD**	**t-test**	**Wilcoxon Z**
REC	3.89	3.89	0.51	3.58	3.60	0.54	−74.7***	−74.1
%Buy	57.2%			42.1%				
%Sell	2.6%			5.2%				
V_{RI1}/P	0.62	0.55	0.49	0.71	0.66	0.36	28.9***	51.2***
V_{RI2}/P	0.65	0.62	0.57	0.89	0.85	0.46	62.3***	86.6***
V_{PEG}/P	0.74	0.87	1.54	0.83	0.82	0.84	10.9***	−13.6***
LTG	20.22	16.97	11.61	15.91	14.53	8.18	−58.6***	−48.8***
SAR	−0.041	−0.0982	0.513	−0.032	−0.104	0.515	1.95*	−0.69
MV	7,270	1,094	24,464	7,663	1,408	24,039	2.22**	20.6***
NUM	10.41	9.00	6.94	10.70	9.00	7.31	5.47***	3.42***

Panel C: Pearson Correlations before (1993–1999) and after (2000–2005) Reg FD[a]

	REC	*SAR*	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*
REC	•	−0.119	−0.195	−0.129	0.228	0.339
SAR	−0.146	•	0.091	0.064	−0.163	−0.267
V_{RI1}/P	−0.127	0.197	•	0.935	0.460	−0.296
V_{RI2}/P	−0.075	0.170	0.888	•	0.543	−0.206
V_{PEG}/P	0.267	−0.017	0.466	0.545	•	0.407
LTG	0.283	−0.350	−0.307	−0.264	0.273	•

Panel D: Pearson Correlations before (2000–2002) and after (2003–2005) OtherReg[b]

	REC	*SAR*	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*
REC	•	−0.168	−0.170	−0.101	0.199	0.233
SAR	−0.115	•	0.209	0.188	−0.001	−0.411

(*continued on next page*)

TABLE 1 (continued)

	REC	*SAR*	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*
V_{RI1}/P	−0.003	0.178	•	0.918	0.506	−0.305
V_{RI2}/P	0.113	0.148	0.860	•	0.603	−0.265
V_{PEG}/P	0.324	−0.053	0.460	0.584	•	0.136
LTG	0.269	−0.225	−0.267	−0.185	0.413	•

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[a] Pearson correlations before (after) Reg FD are above (below) the diagonal.

[b] Pearson correlations before (after) other regulations are above (below) the diagonal.

Variable Definitions:

REC = mean consensus analyst recommendation: 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, 5 = Strong Buy;

%Buy = percentage of recommendations rated Buy or Strong Buy;

%Sell = percentage of recommendations rated Sell or Strong Sell;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption;

V_{PEG} = forecasted earnings per share for a two-year forecast horizon times *LTG* (× 100);

LTG = consensus (median) projected long-term growth in earnings;

P = share price on the date of the consensus recommendation calculation;

SAR = annual size-adjusted return beginning the month following the recommendation;

MVE = market value of equity; and

NUM = number of analysts following.

decreases from 57.2 to 42.1, and the percentage of sell and strong sell recommendations increases from 2.6 to 5.2 percent. V_{RI1}/P, V_{RI2}/P, and V_{PEG}/P increase significantly, but *LTG* forecasts decrease significantly from 20.2 percent to 15.9 percent. These results suggest that the major decreases in analysts' recommendations and *LTG* projections appear following other regulations.

Panels C and D of Table 1 provide correlations between variables. Consistent with the intent of regulations, the correlations between residual income valuations (scaled by current price) and stock recommendations increase over time. However, there is an increase in the positive correlation between V_{PEG}/P and recommendations, even though the correlation between V_{PEG}/P and future returns becomes insignificant post-Reg FD and then becomes negative after other regulations. The correlation between residual income valuations and future returns is increasing, but that improvement occurs only around Reg FD. *LTG* forecasts and residual income valuations are negatively correlated, explaining why residual income valuations and future returns are positively correlated, while *LTG* forecasts and future returns are negatively correlated.

V. REGRESSION RESULTS

As in Bradshaw (2004), each coefficient reported in the tables represents the mean coefficient from 12 subsample regressions. The 12 subsamples are created by partitioning all observations based on one-year-ahead earnings forecast horizons (i.e., months $t-1$ to $t-12$). This controls for systematic differences in earnings forecast characteristics as the end of the period nears (Brown 2001; Bradshaw 2004). It is an empirical regularity that analysts walk down their forecasts as the year passes, and forecasts made near the end of the year are more accurate and less optimistic than those made near the beginning of the year. By running the regression for each fiscal month, we prevent mixing short-horizon earnings forecasts with long-horizon forecasts. In other words, we prevent mixing valuation estimates generated from more optimistic, less accurate forecasts (i.e., long-horizon forecasts) with those generated from less optimistic, more accurate forecasts (i.e., short-horizon forecasts).[26] Reported t-statistics are based on the standard error of the monthly coefficients, using the adjustment for serial correlation across months.[27,28]

The adjusted R^2s presented are means across the 12 months. We estimate the regressions using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each month based on the distribution of the variable in that month. The quintile rankings are scaled to range between 0 and 1.[29]

Tests of Effects of Regulatory Reforms on Relations between Stock Recommendations and Valuation Estimates (Hypothesis 1)

To test the effect of Reg FD on the relation between valuation estimates and stock recommendations, we estimate the following model:

$$REC = \alpha_0 + \alpha_1 RegFD + \alpha_2 VALUATION + \alpha_3 VALUATION * RegFD + \varepsilon \qquad (6)$$

where *VALUATION* is one of the four valuation estimates and *RegFD* is an indicator variable that takes the value of 1 following implementation of Reg FD, and 0 otherwise. α_2 provides an estimate of the relation between recommendations and valuations in the pre-Reg FD period. If α_3 is greater (less) than zero, then the relation between recommendations and valuations has increased (decreased) following Reg FD.

Table 2 presents regression results. Contrary to what one might expect but consistent with Bradshaw's (2004) 1994–1998 results, the table shows that analysts' recommendations are positively related to heuristic-based valuation estimates but are negatively related to more rigorous residual income valuations in the pre-Reg FD period. Directly related to H1, we find that the interactions of both V_{RI1}/P and V_{RI2}/P with *RegFD* are positive and significant at the 1 percent level. These findings support the first hypothesis that Reg FD will better align analysts' recommendations with residual income valuations, which were developed using analysts' earnings forecasts. Also consistent with H1, we find that recommendations are significantly less positively associated with *LTG* following Reg FD (i.e., the interaction term is negative and significant at the 1 percent level), suggesting a reduced reliance on *LTG*. However, in contrast to our prediction, the relation between stock recommendations and *PEG* valuation slightly increases following Reg FD.[30] In conclusion, for

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[26] As an example of this issue, we find that V_{RI1}/P uniformly decreases over the 12-month horizon. The mean of V_{RI1}/P is 12 percent lower in month t–1 compared to month t–12. The same decreasing pattern is observed for V_{RI2}/P (14 percent lower in month t–1) and V_{PEG}/P (24 percent lower in month t–1). Thus, Bradshaw's (2004) approach directly controls for this horizon effect in analysts' forecasts.

[27] Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1 + \Phi)}{(1 - \Phi)} - \frac{2\Phi(1 - \Phi^n)}{n(1 - \Phi)^2}}$, where n is the number of months and Φ is the first-order autocorrelation of the monthly coefficient estimates (Abarbanell and Bernard 2000; Bradshaw 2004).

[28] Since each of the fiscal-month regressions contains multiple observations for the same firm, there is likely some residual dependence, understating the standard error in each of the monthly regressions. However, the monthly coefficients are unbiased. And since we base our reported t-statistics on the mean of the monthly coefficients (not the monthly standard errors), the reported significance levels are unaffected.

[29] We have also estimated the models using five-group, three-group, and two-group (above/below median) ordered logit regressions. Untabulated results show that no inferences are affected with these alternative estimation techniques.

[30] Coefficient estimates in the post-Reg FD period are as follows (untabulated): V_{RI1}/P is significantly negative, V_{RI2}/P is not significantly different from zero, and V_{PEG}/P and *LTG* are significantly positive.

TABLE 2
Relation between Recommendations and Valuation Estimates before (1993–1999) and after (2000–2005) Reg FD

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	4.009***	3.954***	3.635***	3.536***
	(385.8)	(247.6)	(280.1)	(1891.9)
RegFD	−0.262***	−0.279***	−0.151***	−0.043*
	(−7.53)	(−7.58)	(−6.53)	(−1.89)
V_{RI1}/P	−0.304***			
	(−7.75)			
V_{RI2}/P		−0.186***		
		(−4.69)		
V_{PEG}			0.382***	
			(24.1)	
LTG				0.625***
				(103.2)
$V_{RI1}/P * RegFD$	0.187***			
	(5.52)			
$V_{RI2}/P * RegFD$		0.225***		
		(6.07)		
$V_{PEG}/P * RegFD$			0.065**	
			(2.02)	
LTG * *RegFD*				−0.214***
				(−16.9)
Adjusted R^2	0.109	0.096	0.145	0.193

*, **, *** Significant at the 0.10, 0.05, and 0.01 level, respectively, based on two-tailed t-tests.
The table presents the results of regressions of consensus stock recommendations on valuation estimates. Regressions are estimated based on one-year-ahead earnings forecast horizon (i.e., months $t-1$ to $t-12$). The table presents mean coefficients for these 12 monthly regressions. t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\Phi)}{(1-\Phi)} - \frac{2\Phi(1-\Phi^n)}{n(1-\Phi)^2}}$, where n is the number of months and Φ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4)).
RegFD = 1 if an observation is in the post-Reg FD period (2000–2005), and 0 otherwise (1993–1999).
Other variables are defined in Table 1.

three of the four models the results provide support for the first hypothesis, suggesting significant effects of Reg FD on the association between analyst recommendations and valuation estimates.

For our test of the effects of other regulations, we estimate a similar model but limit the sample period to the post-Reg FD era and repeat the above test after replacing *RegFD* with *OtherReg*, an indicator variable that takes the value of 1 for the 2003–2005 period (post-OtherReg), and 0 for the 2000–2002 period (pre-OtherReg).

$$REC = \alpha_0 + \alpha_1 OtherReg + \alpha_2 VALUATION + \alpha_3 VALUATION * OtherReg + \varepsilon. \quad (7)$$

Table 3 presents regression results. The coefficients on V_{RI1}/P and V_{RI2}/P are significantly negative, indicating that residual income valuations remain significantly negatively related to recommendations after Reg FD but before other regulations. The relation between residual income valuations and recommendations becomes significantly more positive after other regulations, as indicated by their interactions with *OtherReg*. These results are consistent with the first hypothesis. In fact, untabulated results show that the coefficient on

TABLE 3

Relation between Recommendations and Valuation Estimates before (2000–2002) and after (2003–2005) Other Regulations (OtherReg)

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	4.022*** (760.5)	3.982*** (661.5)	3.805*** (537.3)	3.733*** (437.1)
OtherReg	−0.346*** (−9.15)	−0.412*** (−8.46)	−0.378*** (−9.48)	−0.283*** (−24.6)
V_{RI1}/P	−0.206*** (−8.90)			
V_{RI2}/P		−0.093*** (−4.33)		
V_{PEG}/P			0.309*** (40.1)	
LTG				0.347*** (15.8)
$V_{RI1}/P * OtherReg$	0.206*** (12.3)			
$V_{RI2}/P * OtherReg$		0.293*** (24.2)		
$V_{PEG}/P * OtherReg$			0.298*** (20.5)	
*LTG * OtherReg*				0.110*** (8.08)
Adjusted R^2	0.102	0.292	0.165	0.150

*, **, *** Significant at the 0.10, 0.05, and 0.01 level, respectively, based on two-tailed t-tests.

The table presents the results of regressions of consensus stock recommendations on valuation estimates. Regressions are estimated based on one-year-ahead earnings forecast horizon (i.e., months $t-1$ to $t-12$). The table presents mean coefficients for these 12 monthly regressions. t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\Phi)}{(1-\Phi)} - \frac{2\Phi(1-\Phi^n)}{n(1-\Phi)^2}}$, where n is the number of months and Φ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4)).

OtherReg = 1 if an observation is in the post-other regulation period (2003–2005), and 0 otherwise (2000–2002).

Other variables are defined in Table 1.

V_{RI1}/P is indistinguishably different from zero in the post-OtherReg period and the coefficient on V_{RI2}/P becomes significantly positive. Thus, it appears that other regulations have played a greater role than has Reg FD in aligning residual income valuations and analysts' recommendations. At least with respect to V_{RI2}/P, the puzzling negative relation between residual income valuations and recommendations now appears to be positive, as one might expect prior to observing results in prior literature.

Contrary to our first hypothesis, we do not detect a decline in the relation between *REC* and heuristics (*LTG* and V_{PEG}/P) after other regulations. The relation between *REC* and V_{PEG}/P continues to increase. The relation between *REC* and *LTG* also increases after having been reduced immediately following Reg FD.

To summarize, the results in Tables 2 and 3 suggest that recent regulations have had an effect on analyst behavior. Specifically, we document a greater reliance on residual income valuations in arriving at stock recommendations following recent regulations. These results are consistent with the objectives of Reg FD and the other regulations and provide support for H1. However, the results for the effects of regulations on heuristics-based valuation estimates (V_{PEG}/P and *LTG*) are mixed for Reg FD and contrary to expectations for other regulations.

Tests of Relations between Future Excess Returns and Valuation Estimates (Hypothesis 2) and Stock Recommendations (Hypothesis 3)

We now turn to testing the relation of future excess returns with both valuation estimates and stock recommendations. We compute one-year-ahead buy-and-hold size-adjusted returns (*SAR*) as:

$$SAR_i = \left[\prod_{\tau=1}^{12} (1 + r_{i,t+\tau}) - \prod_{\tau=1}^{12} (1 + r_{size,t+\tau}) \right], \tag{8}$$

where $r_{i,t+\tau}$ is the monthly raw stock return for firm i in month $t+\tau$, and $r_{size,t+\tau}$ is the month $t+\tau$ return of the size decile to which firm i belongs as of the beginning of the fiscal year. Using I/B/E/S price and dividend data (supplemented with Compustat data), we cumulate returns beginning in the month subsequent to the date of the consensus recommendation. We chose to use a one-year-ahead return horizon for two reasons. First, this is the horizon employed by Bradshaw (2004) so our results are directly comparable to his. Second, recommendations are generally provided by analysts with the intention of giving guidance over an extended period of time (e.g., 6 to 24 months).

To test the second hypothesis, we run the following regression to estimate the relation between future excess returns and the valuation estimates:

$$SAR = \beta_0 + \beta_1 RegFD + \beta_2 VALUATION + \beta_3 VALUATION * RegFD + \varepsilon. \tag{9}$$

For the third hypothesis, we consider the relation between future returns and stock recommendations.

$$SAR = \beta_0 + \beta_1 RegFD + \beta_2 REC + \beta_3 REC * RegFD + \varepsilon. \tag{10}$$

Panel A of Table 4 shows regression results for Equations (9) and (10). Consistent with the findings of Frankel and Lee (1998) and Bradshaw (2004), we document that both V_{RI1}/P and V_{RI2}/P are positively and significantly related to future excess returns before

TABLE 4
Relation between Annual Size-Adjusted Returns and Stock Recommendations and Valuation Estimates before (1993–1999) and after (2000–2005) Reg FD

Panel A: Individual Effects

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	−0.095*** (−13.5)	−0.073*** (−7.91)	0.173*** (18.2)	0.246*** (29.2)	0.531*** (29.9)
RegFD	−0.051 (−1.64)	−0.055 (−1.68)	−0.161 (−5.17)	−0.005 (−0.10)	0.067*** (0.69)
V_{RI1}/P	0.176*** (12.4)				
V_{RI2}/P		0.124*** (7.69)			
V_{PEG}/P			−0.310*** (−11.2)		
LTG				−0.501*** (−30.4)	
REC					−0.139*** (−33.6)
V_{RI1}/P * *RegFD*	0.148*** (3.36)				
V_{RI2}/P * *RegFD*		0.175*** (3.50)			
V_{PEG}/P * *RegFD*			0.280*** (7.35)		
LTG * *RegFD*				−0.061 (−1.34)	
REC * *RegFD*					−0.019 (−0.89)
Adjusted R^2	0.019	0.014	0.022	0.088	0.018

Panel B: Incremental Effects

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	0.401*** (34.5)	0.452*** (37.0)	0.526*** (24.4)	0.306*** (22.9)
RegFD	−0.009 (−0.16)	0.020 (0.33)	0.084 (0.75)	0.208 (2.60)**
V_{RI1}/P	0.137*** (10.7)			
V_{RI2}/P		0.098*** (6.70)		
V_{PEG}			−0.271*** (−9.15)	
LTG				−0.490*** (−28.2)
REC	−0.123*** (−39.1)	−0.132*** (−38.2)	−0.097*** (−13.1)	−0.017*** (−4.44)

(*continued on next page*)

TABLE 4 (continued)

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
$V_{RI1}/P * RegFD$	0.166*** (4.49)			
$V_{RI2}/P * RegFD$		0.199*** (4.69)		
$V_{PEG}/P * RegFD$			0.304*** (6.54)	
$LTG * RegFD$				−0.043 (−1.02)
$REC * RegFD$	−0.014 (−1.08)	−0.023 (−1.56)	−0.065** (−2.26)	−0.056*** (−5.90)
Adjusted R^2	0.032	0.029	0.033	0.089

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*, **, *** Significant at the 0.10, 0.05, and 0.01 level, respectively, based on two-tailed t-tests.
The table presents the results of regressions of buy-and-hold annual size-adjusted returns on valuation estimates and consensus stock recommendations. Regressions are estimated based on one-year-ahead earnings forecast horizon (i.e., months $t-1$ to $t-12$). The table presents mean coefficients for these 12 monthly regressions. t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\Phi)}{(1-\Phi)} - \frac{2\Phi(1-\Phi^n)}{n(1-\Phi)^2}}$, where n is the number of months and Φ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4)).
RegFD = 1 if an observation is from the post-Reg FD period (2000–2005,) and 0 otherwise (1993–1999).
Other independent variables are defined in Table 1.

Reg FD. In addition, we find that this positive relation increases following Reg FD (and in fact doubles). These results provide support for the second hypothesis. The coefficients on *LTG* and V_{PEG}/P are *negatively* related to future excess returns prior to Reg FD. The introduction of Reg FD did appear to make V_{PEG}/P significantly less negatively related to future returns (i.e., the interaction is positive and significant at the 1 percent level). For *LTG*, on the other hand, there is no significant effect of Reg FD. The final column of Panel A in Table 4 shows that recommendations are negatively related to future excess returns. After enactment of Reg FD, this negative relation persists. This suggests that Reg FD had no impact on the seemingly irrational relation between analyst recommendations and security returns.

In Panel B of Table 4, we examine whether valuations are incremental to stock recommendations. As discussed previously, to the extent that analysts' recommendations are not derived based on valuation models, the two can provide incremental effects. We first note that results for all four valuation estimates (reported in Panel A) and the effects of Reg FD are unaffected by adding recommendations to the regression. This provides further evidence that analysts' stock recommendations are influenced by many other factors. The biggest difference in the pre-Reg FD period is for *LTG*. Much of this variable's explanatory power is lost when testing for an incremental effect, which is consistent with our earlier result that recommendations appear most closely related to *LTG* (as opposed to residual income valuations). Results for the post-Reg FD are also very similar. Perhaps the most interesting result is that when controlling for V_{PEG}/P or *LTG*, the relation between stock recommendations and future excess returns becomes even more negative in the post-Reg FD period. This is not the case for residual income valuations. The ability of residual income valuations to explain future returns prevents the negative relation between recommendations and future returns from becoming increasingly negative.

Table 5 provides analyses of effects of other regulations (OtherReg) on the relations between future returns and valuation estimates and recommendations. The main findings reported in Panel A are as follows. First, the positive relation between residual income valuations and future returns remains the same before and after other regulations. Second, the other regulations do seem to have had an effect on the relation between stock recommendations and future returns, as the interaction effect is significantly positive. These results provide support for the third hypothesis. When we consider the incremental effects of valuations and stock recommendations for future returns (reported in Panel B), only one conclusion changes. The negative relation between stock recommendations and future returns does not become weaker when controlling for *LTG* (i.e., column 4 of Panel B). In general, the results in Table 5 further demonstrate that other regulations relate primarily to improvements in stock recommendations (as opposed to analysts' earnings forecasts) and this improvement is incremental to valuation estimates based on analysts' earnings forecasts.

Sensitivity Analyses

Results for Observations with No Change in Consensus

Recall that we base our results on using only monthly observations for which there has been a revision in the consensus recommendation. We use these observations to be consistent with Bradshaw (2004). However, as a sensitivity analysis, we repeat the tests using the full sample of observations from I/B/E/S data (i.e., including monthly observations with no change in consensus recommendation). This approach has the advantage of significantly increasing the sample size and thus the power of our tests. In fact, the sample size increases to 425,128. However, the results are quite similar to those reported previously, which provides some assurance that our findings are not unduly influenced by the use of a smaller sample.

Standard Errors Adjusted for Clustering at the Firm Level

In Tables 2–5 we report coefficients using the mean coefficient from 12 fiscal-month regressions. As an alternative, we consider estimating coefficients using a pooled model and use firm-cluster-adjusted standard errors. The pooled model has the disadvantage (as discussed previously) of mixing long-horizon and short-horizon earnings forecasts but the advantage of not relying on the average of only 12 monthly coefficients, which potentially reduces statistical power. Under this alternative approach, we find that coefficients are remarkably close to those reported in the tables. All conclusions reported from Tables 2 and 3 (i.e., the relations between stock recommendations and the four valuation estimates) are unaffected.

We do, however, notice some differences for results reported in Tables 4 and 5 (i.e., the relations with future returns). *LTG* is significantly more negatively related to future returns after Reg FD, but significantly less negatively related to future returns after other regulations. These results are consistent with other regulations reducing analysts' reliance on heuristics in setting stock recommendations. Furthermore, the conclusion that the increasing positive relation between residual income valuations and future returns is attributable primarily attributable to Reg FD (and not other regulations) is even more apparent. In summary, while we note some differences in results, overall conclusions regarding the effectiveness of regulations are unaffected.

TABLE 5
Relation between Annual Size-Adjusted Returns and Stock Recommendations and Valuation Estimates before (2000–2002) and after (2003–2005) Other Regulations (OtherReg)

Panel A: Individual Effects

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	−0.160*** (−18.6)	−0.145*** (−14.7)	−0.051*** (−7.2)	0.349*** (7.8)	0.674*** (7.2)
OtherReg	0.121*** (6.12)	0.143*** (4.43)	0.147*** (7.47)	−0.128 (−1.40)	−0.190*** (−3.61)
V_{RI1}/P	0.344*** (7.47)				
V_{RI2}/P		0.329*** (6.19)			
V_{PEG}/P			0.005 (0.24)		
LTG				−0.652*** (−9.72)	
REC					−0.182*** (−9.20)
V_{RI1}/P * *OtherReg*	−0.054 (−0.65)				
V_{RI2}/P * *OtherReg*		−0.083 (−0.76)			
V_{PEG}/P * *OtherReg*			−0.102** (−2.63)		
LTG * *OtherReg*				0.283* (1.93)	
REC * *OtherReg*					0.074*** (3.67)
Adjusted R^2	0.045	0.038	0.004	0.135	0.027

Panel B: Incremental Effects

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	0.449*** (8.03)	0.527*** (8.75)	0.680*** (6.97)	0.620*** (7.11)
OtherReg	−0.069 (−0.90)	−0.057 (−0.76)	−0.197*** (−3.68)	−0.169 (−2.10)**
V_{RI1}/P	0.310*** (7.36)			
V_{RI2}/P		0.310*** (6.28)		
V_{PEG}/P			0.063** (1.96)	
LTG				−0.626*** (−8.61)
REC	−0.151*** (−11.6)	−0.168*** (−11.9)	−0.191*** (−7.77)	−0.072*** (−2.79)

(*continued on next page*)

TABLE 5 (continued)

	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
V_{RI1}/P * *OtherReg*	−0.017 (−0.20)			
V_{RI2}/P * *OtherReg*		−0.037 (−0.37)		
V_{PEG}/P * *OtherReg*			−0.100** (−2.31)	
LTG * *OtherReg*				0.281 (1.62)
REC * *OtherReg*	0.042* (2.02)	0.042*** (2.70)	0.090*** (3.96)	0.010 (0.17)
Adjusted R^2	0.063	0.061	0.029	0.142

*, **, *** Significant at the 0.10, 0.05, and 0.01 level, respectively, based on two-tailed t-tests.

The table presents the results of regressions of buy-and-hold annual size-adjusted returns on valuation estimates and consensus stock recommendations. Regressions are estimated based on one-year-ahead earnings forecast horizon (i.e., months $t-1$ to $t-12$). The table presents mean coefficients for these 12 monthly regressions. t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\Phi)}{(1-\Phi)} - \frac{2\Phi(1-\Phi^n)}{n(1-\Phi)^2}}$, where n is the number of months and Φ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4)).

OtherReg = 1 if an observation is in the post-other regulation period (2003–2005), and 0 otherwise (2000–2002).

Other variables are defined in Table 1.

Bear Market and Bull Market Effects

Our research period can be characterized by periods of primarily a bull market until March 2000, bear market from April 2000 through March 2003, and another bull market commencing in April 2003. To test whether our inferences are affected by bull versus bear markets in addition to the effects of regulatory reforms, we re-estimate regressions using bull or bear monthly indicators.[31] The overall tenor of our results is the same. We do find that bull markets have a positive effect on analysts' recommendations and excess returns in the pre-Reg FD.

VI. CONCLUSION

To date there has been surprisingly little research on analysts' recommendations and analysts' use of valuation models. *A priori*, the relation seems straightforward. Analysts input their earnings forecasts into the theoretically correct valuation model, such as a residual income model, to develop a valuation estimate. Analysts compare this valuation to current stock price. To the extent that the valuation estimate exceeds current stock price, analysts would issue a buy recommendation. Alternatively, if the valuation estimate is below the current stock price, then analysts would issue a sell recommendation. Thus, it seems likely that residual income valuations (scaled by current price) and stock recommendations would have a positive relation and each would relate positively to future returns. Furthermore, if stock recommendations completely capture the information in valuation estimates, then valuation estimates would have no incremental explanatory power for future returns. However, while these arguments seem consistent with rational analyst behavior, prior research documents that these relations do not exist as expected and in some cases exist in the opposite direction.

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[31] For the entire 1993–2005 research period, we use a monthly indicator that equals 1 during bull markets, and 0 during the bear markets. We also use the monthly indicator for separate analysis during the post-Reg FD periods (2000–2005) and find no significant effects.

As an example, Bradshaw (2004) shows that residual income valuations, developed using analysts' earnings forecasts, relate negatively to analysts' recommendations yet relate positively to future returns. Why are analysts' earnings forecasts in residual income valuation models useful to investors (i.e., help in predicting future stock performance), but yet analysts do not appear to use them in setting their recommendations? In other words, why do analysts not practice (recommend) what they preach (forecast)?

Because of these inconsistencies (along with the crash of technology stocks in the early 2000s), analyst activity has come under severe public scrutiny. Regulators were called upon to "fix" the analyst industry. The SEC enacted Regulation Fair Disclosure (Reg FD) in 2000, which prohibited management from disclosing material information to selected analysts. Some contend that analysts purposely biased their forecasts to gain favor with management, thereby allowing easier access to privileged information. Reg FD disallows the release of privileged information and therefore reduces at least one of the incentives for analysts to bias their forecasts.

Analysts were also criticized for the apparent conflict of interest that existed within brokerage firms. Analysts in the research department (i.e., those providing stock recommendations) felt pressure from those in the investment banking department to provide only favorable reports. Issuance of unfavorable reports could reduce investment banking business, a tremendous source of revenue for brokerage firms. Thus, analysts had incentives in issuing their recommendations beyond providing objective, reliable information to the investing public. In response, the SEC accepted NASD Rule 2711, NYSE Rule 472, and the Global Research Analyst Settlement in late 2002 and 2003. In general, these regulations address research analysts' conflicts of interest and limit interactions and flow of information between an analyst and the investment banking business of the brokerage firm.

We are interested in the extent to which these regulations had their intended effects. Using a large sample of stock recommendations over the 1993–2005 period, we first examine the relation between analysts' stock recommendations and (1) theoretically derived residual income models versus (2) valuation heuristics (i.e., price-to-earnings to growth [PEG] ratio and long-term growth [LTG] forecast). We then examine the relation between future returns and (1) stock recommendations, (2) residual income models, and (3) valuation heuristics. Finally, we consider the extent to which residual income models and valuation heuristics are incremental to stock recommendations in explaining future returns. We examine changes in these relations in the pre-Reg FD period (1994–1999) versus the post-Reg FD period (2000–2005). Within the post-Reg FD period, we examine changes before (2000–2002) and after (2003–2005) other regulations (i.e., NASD 2711, NYSE Rule 472, and Global Research Analyst Settlement).

We report the following results. The documented negative relation between stock recommendations and residual income valuations diminishes in the post-Reg FD period and even becomes positive following other regulations. We also find evidence of a reduced analyst reliance on long-term growth forecasts in providing a stock recommendation in the post-Reg FD period. For our tests of a relation with future returns, we show that residual income valuations have an increasingly positive relation in the post-Reg FD period. This change is due primarily to Reg FD itself rather than other regulations. This finding implies that Reg FD had the effect of increasing the usefulness of earnings forecasts to investors. Also of interest to investors is our finding that the negative relation between stock recommendations and future returns still persists but is diminishing following regulations subsequent to Reg FD. Thus, it appears that in many ways regulations are having their intended effects, but the effects on analysts' outputs may be incomplete.

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NASD Rule 2711 and Changes in Analysts' Independence in Making Stock Recommendations

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ABSTRACT: This study provides evidence of changes in how analysts generate stock recommendations after the SEC's approval of NASD Rule 2711 in May 2002, which introduced regulatory reforms to enhance the independence of analysts' research. We investigate the relations of analysts' stock recommendations with intrinsic value estimates (based on analysts' earnings forecasts relative to the stock prices, V/P) and with investment-banking-related conflicts of interest during the 1994–2005 period. We find a stronger relation between analysts' stock recommendations and V/P and a weaker relation between analysts' stock recommendations and conflicts of interest in the post-Rule period than prior to the implementation of the Rule. Moreover, the increases in the relation between stock recommendations and V/P after the implementation of the Rule are greater for the stocks recommended by analysts with greater potential conflicts of interest. Our findings suggest that the implementation of Rule 2711 has enhanced analysts' independence.

Keywords: *stock recommendations; earnings forecasts; earnings-based valuation; conflict of interest; NASD Rule 2711.*

Data Availability: *Data used in this study are available from public sources.*

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I. INTRODUCTION

Analysts' conflicts of interest have been scrutinized by the U.S. Congress and the Securities and Exchange Commission (SEC) in recent years.[1] The main concern is that analysts' stock recommendations do not reflect analysts' actual opinions about the investment potential of subject companies but are instead influenced by incentives to generate investment banking business and commission revenues. On May 10, 2002, the SEC approved NASD Rule 2711, *Research Analysts and Research Reports*, which aims to enhance the independence of analysts' research so that investors are provided useful and reliable information with which to make investment decisions. The SEC also approved similar amendments to NYSE Rule 472 on the same day. The rules were implemented in phases during the period from July 9, 2002 to November 6, 2002.[2]

Rule 2711 prohibits NASD and NYSE members from tying analysts' compensation to their company's investment banking transactions and from offering favorable research or a specific rating or price target to a company as consideration or inducement for future business. These prohibitions reflect the belief that linking analysts' compensation to investment banking business diverts analysts' efforts from forming objective valuations and recommendations to currying favors with managers. In addition, Rule 2711 requires disclosure of the percentage of all securities by the member that receive "Buy," "Hold/Neutral," or "Sell" ratings in each research report. Following the implementation of Rule 2711, there were large decreases (increases) in the percentage of Buy (Sell) recommendations (Barber et al. 2006).

In this study, we provide evidence on whether Rule 2711 has enhanced analysts' independence. We infer the degree of analyst independence based on the strength of the relation of analysts' stock recommendations with intrinsic values relative to stock prices and with conflicts of interest. Analysts advise investors on the purchase (sale) of undervalued (overvalued) stocks. Therefore, stock recommendations should be positively related to analysts' estimates of the intrinsic value relative to the current price of the stock. Analysts' private valuations are unobservable, however. Following prior research, we use the residual income model and the price-earnings-to-growth model (denoted by V^{RI} and V^{PEG}, respectively) to proxy for analysts' valuations (Bradshaw 2004). We hypothesize that the relation between analysts' stock recommendations and the intrinsic value estimates relative to stock prices (V^{RI}/P and V^{PEG}/P) is stronger after the implementation of Rule 2711.

Regulators considered analyst compensation linked with current or future underwriting and investment banking services as the primary conflict of interest.[3] Therefore, in our empirical analyses, we focus on this particular type of conflict of interest and consider the demand for and supply of investment banking and underwriting services, measuring demand from the level of a company's net external financing and supply from the amount of underwriting services provided by analysts' employers.[4] We hypothesize that the relation between analysts' stock recommendations and conflicts of interest is weaker after the implementation of Rule 2711.

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[1] In summer 2001, the SEC finished their investigation with a report of their findings entitled "Analyzing the Analysts: Are Investors Getting Unbiased Research from Wall Street?" presented to the House Financial Services Subcommittee on Capital Markets. In the fall of 2001, the SEC called for the National Association of Securities Dealers (NASD) and the New York Stock Exchange (NYSE) to work together to craft new rules addressing analysts' conflicts of interest.

[2] In the rest of the paper, the term Rule 2711 refers to both NASD Rule 2711 and the amended NYSE Rule 472.

[3] Other conflicts of interest identified by the regulators are (1) positive research reports to trigger higher trading volumes and in turn generate larger commissions for the analysts' firms and (2) ownership of equity by the analyst or the brokerage firm in the companies that the analyst covers. See the SEC Acting Chair Laura Unger's written testimony before the Subcommittee on Capital Markets, Insurance, and Government Sponsored Enterprises, Committee on Financial Services, U.S. House of Representatives on July 31, 2001.

Our sample consists of company-month observations for the 1994–2005 period. The empirical results show that the relations between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P are significantly stronger after Rule 2711 than prior to the Rule, suggesting that analysts incorporate a greater extent of the divergence in the current price from the intrinsic value of the stock into their stock recommendations in the post-regulation period, consistent with our first hypothesis. Results also show that the relation between analysts' stock recommendations and the two proxies for conflicts of interest is weaker after the implementation of Rule 2711, suggesting a smaller influence of conflict of interest on analysts' stock recommendations, consistent with our second hypothesis.

To enhance the likelihood that our results are attributable to Rule 2711, we first partition the sample into two groups based on the degree of potential conflicts of interest. We find that before Rule 2711, the relations between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P are weaker for companies that have higher external financing and that are followed by more analysts whose employers are major suppliers of investment banking services than for other companies. However, the increases in the relations between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P after the Rule are also significantly greater when the recommendations are issued by analysts with greater potential conflicts of interest. In other words, the larger increases in the relation of interest obtain for the stock recommendations issued by analysts who are more affected by Rule 2711, which provides further evidence that our results are attributable to the Rule.

We conduct several additional tests to rule out alternative explanations, such as the implementation of Regulation Fair Disclosure (Reg FD), analysts' substitution of investment signals, and changes in market sentiment. We find that the relation of analysts' stock recommendations with V^{RI}/P and V^{PEG}/P in the post-FD but pre-Rule 2711 period is not stronger than that in the pre-FD period. To examine if our results are due to a substitution effect of other investment signals for V^{RI}/P and V^{PEG}/P, we control for the stock return before the recommendations are issued and find that the relations of stock recommendations with V^{RI}/P and V^{PEG}/P are unaffected. Furthermore, we find that the changes in the relations of analysts' stock recommendations with V^{RI}/P and V^{PEG}/P in the post-Rule period are unrelated to the fluctuations in the overall market level (i.e., the return from the S&P 500 Index).

We also test whether our results are due to analysts' decisions to add or drop coverage of certain companies triggered by the new regulation. We analyze both a constant sample and a sample of observations that have no change in analyst following after Rule 2711. For both samples, we find that the relations between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P are both significantly stronger after Rule 2711 than prior to the Rule. Our results are robust to alternative estimates of the terminal value and cost of capital in computing V^{RI} and to alternative specifications (regressions of changes in stock recommendations on changes in V^{RI}/P and V^{PEG}/P).

Our inferences are subject to limitations. Our tests are based on the premise that the intrinsic value estimates capture analysts' private valuations. To the extent that the degree to which those value estimates captured analysts' private valuations changed around the implementation of Rule 2711 for reasons unrelated to the regulation, we cannot necessarily infer a causal effect of regulation. In addition, analysts could have shifted their valuation methods for reasons unrelated to regulatory reforms, which could generate changes in the relation between analyst stock recommendations and value estimates relative to stock prices.

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[4] As discussed below, we use analyst consensus data. Hence, our proxy variables for analysts' conflicts of interest are company-specific rather than analyst-specific.

As supplemental analysis, we investigate whether the investment value of analysts' stock recommendations increases in the post-Rule period. We find that the portfolio of stocks with Strong Buy recommendations is the only one that earns a significantly positive abnormal return during the post-Rule period, which suggests that the investment value of analysts' Strong Buy recommendations is enhanced after the implementation of Rule 2711.

Our findings contribute to the understanding of analysts' use of their earnings forecasts in making stock recommendation decisions (Schipper 1991; Brown 1993). Our findings also have implications for regulators and investors. In particular, we find that in the pre-Rule period, analysts did not incorporate V^{RI}/P (which is based on their own earnings forecasts) into stock recommendations even though V^{RI}/P could predict future returns. In the post-Rule period, however, analysts incorporate V^{RI}/P into stock recommendations, while V^{RI}/P continues to predict future returns. To the extent that investors view the consistency between analysts' stock recommendations and their earnings forecasts as an attribute of analysts' independence, our results imply that Rule 2711 enhances investors' confidence.

Section II discusses related literature and develops our research hypotheses. Section III lays out the research design, while Section IV describes the sample. Section V presents the main empirical results. Section VI presents the results from tests of alternative explanations and robustness checks. Section VII concludes the paper.

II. RELATED LITERATURE AND HYPOTHESIS DEVELOPMENT

Analysts' Stock Recommendations and Firm Value Estimates

Analysts make forecasts on earnings, cash flows, and growth rates and ultimately issue a stock recommendation or a target price that reflects their opinions about the investment value of the company. Since analysts' primary role is to advise investors on whether a stock is undervalued or overvalued, analysts' stock recommendations should be related to their valuations relative to current stock prices.

Several prior studies examine the link between analysts' valuations and their earnings forecasts. Bandyopadhyay et al. (1995) find that analysts' near-term (long-term) earnings forecast revisions explain about 30 (60) percent of the variation in target price revisions, which suggests that target prices are positively related to earnings expectations. Bradshaw (2002) examines a sample of 103 analyst reports and finds that target prices are a function of analysts' forecasts of earnings and long-term earnings growth rates.

To shed light on what valuation methods analysts use, Asquith et al. (2005) examine the content of 1,126 analyst reports issued during 1997–1999. They document that 99.1 percent of analysts mention the use of some kind of earning multiples, 12.8 percent claim to use some variation of discounted cash flow, and only seven of all the reports use the price-earnings-to-growth ratio as their valuation method. The predominant use of earnings forecasts in valuing stocks as documented by Bradshaw (2002) and Asquith et al. (2005) suggests that analysts' earnings forecasts, valuations, and stock recommendations are linked in a predictable manner and intrinsic value estimates based on analysts' earnings forecasts can be used to mimic analysts' private valuations.

To understand how analysts generate their stock recommendations, Bradshaw (2004) examines the relations of analysts' stock recommendations with intrinsic value estimates relative to the stock prices and with the analysts' forecasts of long-term earnings growth rates. He constructs intrinsic value estimates based on the residual income model and the price-earnings-to-growth (PEG) model (denoted by V^{RI} and V^{PEG}, respectively) with analysts' earnings forecasts as inputs, and uses these estimates to proxy for analysts' valuations. Among the three valuation models or metrics he examines, the residual income model has the most theoretical support and can accommodate a wide range of valuation models such as discounted cash flows or dividends (Ohlson 1995). Compared with the residual income model, the PEG model seems to be more heuristic in the sense that it incorporates only projections of earnings and growth, but Easton (2004) shows that the PEG ratio can be used as a parsimonious way to rank stocks. Analysts' forecasts of long-term growth rates are the most heuristic since these forecasts have the least theoretical support as a stand-alone firm value estimate and they ignore whether growth projections are reflected in stock prices.

Bradshaw (2004) finds that, during the period from January 1994 to June 1998, analysts' stock recommendations are either negatively related or unrelated to the intrinsic values of the stocks relative to the stock prices based on the residual income model. However, the relation is positive for valuation estimates based on the PEG model and for analysts' forecasts of long-term growth rates. Bradshaw (2004) also finds that residual income valuations are positively related to future excess returns, consistent with Frankel and Lee (1998), but long-term growth rates are negatively related to future excess returns, consistent with La Porta (1996) and Dechow and Sloan (1997). In addition, analysts' recommendations are uncorrelated with future excess returns. Overall, the results in Bradshaw (2004) show that analysts do not make use of residual income valuations even though such valuations (relative to stock prices) predict positive future excess returns.

Bradshaw's (2004) finding for analysts' long-term growth forecasts is consistent with Jegadeesh et al. (2004), who find that analysts prefer "glamour" stocks to "value" stocks, which runs counter to the interests of investors. Both Bradshaw (2004) and Jegadeesh et al. (2004) point out that this finding could be indicative of conflicts of interest, because growth firms could potentially bring in more investment banking business and trading commissions.

Given increased public concern about independence of analysts' research and the introduction of Rule 2711, we analyze whether the Rule has enhanced analysts' independence. We infer analyst independence based on the relation between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P. Incorporation of intrinsic value estimates relative to stock prices into stock recommendations is not only consistent with analysts' primary role of detecting undervalued and overvalued stocks, but also in line with investors' interests, as V^{RI}/P and V^{PEG}/P predict future stock returns (Frankel and Lee 1998; Bradshaw 2004). Therefore, the strength of the relations between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P reflects the degree of analysts' independence in generating their stock recommendations.

Barber et al. (2006) document large decreases (increases) in the percentage of Buy (Sell) recommendations shortly after Rule 2711 became effective, suggesting changes in how analysts generate stock recommendations. If those changes are driven by analysts' reorientation to provide investors with unbiased advice, then the relation between analysts' recommendations and the intrinsic value estimates relative to stock prices would be stronger after Rule 2711 than before.

Based on the above discussion, our first hypothesis is as follows:

H1: The relation between analysts' stock recommendations and intrinsic value estimates relative to the stock prices is stronger after the implementation of Rule 2711.

Analysts' Stock Recommendations and Conflicts of Interest

Prior studies find that affiliated analysts issue more favorable stock recommendations than do unaffiliated analysts, where affiliated analysts are defined as analysts working for brokerage firms that have investment banking ties to the companies the analysts cover (Dugar and Nathan 1995; Lin and McNichols 1998; Michaely and Womack 1999). Similarly, O'Brien et al. (2005) find that affiliated analysts are slower to downgrade from "Buy" and "Hold" recommendations and faster to upgrade from "Hold," suggesting that investment banking ties increase analysts' reluctance to reveal negative opinions about the companies that they cover. Kolasinski and Kothari (2007) examine analyst stock recommendations issued around M&A deals and find that analysts affiliated with acquirers are more likely than unaffiliated analysts to upgrade their recommendation of the acquirer around M&A deals. They also find that target-affiliated analysts tend to upgrade acquirers in large all-stock deals after exchange ratios are set, thereby potentially benefiting target managers and shareholders.

While analysts' affiliations likely reflect conflicts of interest arising from recent investment banking ties, unaffiliated analysts may also have incentives to issue favorable research reports to induce future investment banking business. Therefore, to the extent that analysts' research reports are influenced by the prospect of generating future investment banking business, the amount of cash a company is expected to raise from the capital markets in the future may indicate potential conflicts of interest. Bradshaw et al. (2006) find that over-optimism in analysts' forecasts and stock recommendations is positively related to a company's net external financing, and companies with greater net external financing tend to have lower future stock returns. They also find that external financing activity is more important than analysts' affiliations in explaining analyst optimism.

Our second research question is whether conflicts of interest are less influential on analysts' stock recommendations after the implementation of Rule 2711. Because our analysis is based on consensus analyst data, we use company-level (instead of analyst-level) proxies for conflicts of interest. Specifically, we focus on the conflicts of interest induced by future investment banking opportunities and investigate their relation to analysts' stock recommendations.[5] We use net external financing as a proxy for the demand for investment banking services and the amount of underwriting business of the analysts' employers as a proxy for the supply of investment banking services (detailed descriptions below). Compared with Kadan et al. (2008) and Ertimur et al. (2007), who investigate changes in analyst independence after the regulatory reforms based on the profitability of analysts' stock recommendations, we test directly the changes in the influence of investment banking-related conflicts of interest on stock recommendations. Our second hypothesis is as follows:

> **H2:** The relation between analysts' stock recommendations and conflicts of interest is weaker after the implementation of Rule 2711.

III. RESEARCH DESIGN

Analysts' Stock Recommendations and Valuation Estimates

We use the residual income model (e.g., Ohlson 1995; Frankel and Lee 1998) to estimate the intrinsic value of a stock. The intrinsic value per share of stock i estimated in month t is computed as follows:

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[5] Commissions generated from trading activities are another possible source of conflicts of interest (Agrawal and Chen 2005; Cowen et al. 2006). We examine this issue in additional analyses in Section VI.

$$\begin{aligned} V_{it}^{RI} &= BVPS_{it} + \sum_{\tau=1}^{5} \frac{E_t(RI_{it+\tau})}{(1 + r_{it})^{\tau}} + \frac{E_t(TV_{it+5})}{(1 + r_{it})^{5}} \\ &= BVPS_{it} + \sum_{\tau=1}^{5} \frac{E_t(RI_{it+\tau})}{(1 + r_{it})^{\tau}} + \frac{E_t(RI_{it+5})}{r_{it}(1 + r_{it})^{5}}, \end{aligned} \tag{1}$$

where $BVPS$ is the book value of common equity per share, r is the cost of capital, RI is the residual income per share, and TV is the terminal value per share. The expected value of $RI_{t+\tau}$ equals analysts' consensus forecast of τ-year-ahead earnings per share minus $r_{it}*E_t(BVPS_{t+\tau-1})$, where $E_t(BVPS_{t+\tau-1})$ is the future book value per share extrapolated from historical book values using the clean surplus relation and assuming that the firm will continue its recent dividend payout policy (Frankel and Lee 1998).[6] Details of variable definitions are in Section IV.[7]

To investigate whether analysts' stock recommendations are related to the intrinsic value relative to the price of the stock, we estimate the following equation:

$$REC_{it} = \beta_{0t} + \beta_{1t}(V^{RI}/P)_{it} + \varepsilon_{it}, \tag{2}$$

where REC_{it} is the analysts' consensus (mean) recommendation, ranging from 1 (Strong Sell) to 5 (Strong Buy),[8] for company i issued in month t, and $(V^{RI}/P)_{it}$ is the intrinsic value, V_{it}^{RI}, computed in Equation (1) divided by the closing stock price on the day before the consensus recommendation date.

The second valuation estimate we use is based on the product of forecasted earnings and the forecasted growth rate relative to the stock price, V^{PEG}/P. Consistent with Bradshaw (2004), V_{it}^{PEG} equals analysts' consensus forecast of two-year-ahead earnings per share for company i in month t multiplied by the analysts' forecast of the long-term growth rate in earnings (LTG_{it}), and P_{it} is as defined previously. The equation we estimate is:

$$REC_{it} = \beta_{0t} + \beta_{1t}(V^{PEG}/P)_{it} + \varepsilon_{it}. \tag{3}$$

Also consistent with Bradshaw (2004), we use quintile rankings of V^{RI}/P and V^{PEG}/P, scaled to range between 0 and 1, in the regressions. Because the analysts' consensus recommendations and forecasts are available monthly, we estimate Equations (2) and (3) by month and test whether the means of the monthly coefficients on the valuation estimates are significantly different between the pre-Rule and post-Rule periods. In addition, we estimate the following pooled regression:

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[6] We require the sample observations to have earnings forecasts with one-year and two-year horizons. If longer horizon forecasts are not available, then they are extrapolated by applying analysts' forecasts of long-term growth rates to the most recent available forecast. In addition, we assume that the residual income of year $t+5$ persists in perpetuity. Our conclusions in the paper are similar when we assume that there is mean-reversion for the residual income of year $t+5$ (discussed in Section VI).

[7] Note that the forecasts of earnings and residual income are on an annual basis, and the intrinsic value, V_{it}^{RI}, is computed for each month based on the available information (earnings forecasts, book value of common equity, etc.) in that month. Therefore, while the time subscript t alone denotes the month, $t+\tau$ denotes the year (except for $BVPS_t$, which is the most recent year-end book value of common equity before month t).

[8] Individual analysts' stock recommendations reported in I/B/E/S include Strong Buy, Buy, Hold, Underperform, and Sell, which are coded as 1 to 5, respectively. We label the "Underperform" ("Sell") recommendation as "Sell" ("Strong Sell") and invert the coding to make our interpretations more intuitive (the higher the number, the more positive the recommendation). Specifically, our coding is: 1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy.

$$REC_{it} = \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it}{*}RULE_{it} + \varepsilon_{it}, \tag{4}$$

where *RULE* is an indicator variable that equals 1 if the consensus stock recommendation is issued after the SEC's approval of Rule 2711 (i.e., after May 10, 2002), and 0 otherwise,[9] and *VALUATION* equals either V^{RI}/P or V^{PEG}/P. We are primarily interested in determining whether β_3 in Equation (4) is significantly positive.

Analysts' Stock Recommendations and Conflicts of Interest

We use the company's net external financing and the significance of the underwriting business provided by analysts' employers as proxy variables for conflicts of interest. Consistent with Bradshaw et al. (2006), net external financing (*EXF*) equals the sum of changes in the book value of common equity, preferred stock, and short-term and long-term debt minus net income, all divided by average total assets. We use data for the first fiscal year ending after the consensus recommendation date in month *t* to compute EXF_t so that the value proxies for the expected future net external financing.[10] For data on the significance of the underwriting business, we use the sum of the underwriter prestige rankings of the analysts' employers (constructed by Loughran and Ritter [2004]) divided by the total assets of the companies followed by the analysts (denoted as *IB*).[11] The prestige rankings range from 0 for brokerage firms without investment banking businesses to 9 for lead or co-managing underwriters or those that always appear in the highest bracket among non-managing underwriters in the prospectus (see Loughran and Ritter 2004, Appendix C). A higher ranking indicates that the brokerage firm engages in larger amounts of underwriting business and thus has greater incentives to retain or attract clients.

If analysts' stock recommendations are biased due to conflicts of interest, we expect to see more favorable stock recommendations for companies with higher levels of *EXF* and *IB* after controlling for the valuation estimate. Therefore, we estimate the following equations:

$$REC_{it} = \beta_{0t} + \beta_{1t}(V^{RI}/P)_{it} + \beta_{2t}EXF_{it} + \beta_{3t}IB_{it} + \varepsilon_{it}, \tag{5}$$

$$REC_{it} = \beta_{0t} + \beta_{1t}(V^{PEG}/P)_{it} + \beta_{2t}EXF_{it} + \beta_{3t}IB_{it} + \varepsilon_{it}, \tag{6}$$

where all the independent variables equal the monthly quintile ranking scaled to range between 0 and 1. We estimate Equations (5) and (6) by month and test if the means of the monthly coefficients on *EXF* and *IB* are significantly different between the pre-Rule period and the post-Rule period. In addition, we estimate the following pooled regression:

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[9] In this study, we use the approval date of Rule 2711 instead of the effective date to separate the pre- and post-Rule periods because there are different effective dates depending on the provision in the Rule. Most provisions had an effective date of July 9, 2002, while the last date to start disclosure of ratings distributions was September 8, 2002. Our conclusions are unaffected when we use those alternative dates to separate the pre- and post-Rule periods.

[10] Under this definition, EXF_t may also include external financing that occurs before month *t*. For example, if the company's fiscal year ends in December, the variable *EXF* for the observation in June 2004 equals the net external financing during the period from January to December 2004. The results are similar when we compute net external financing using data for the four consecutive quarters starting with the quarter that month *t* belongs to (but the sample size is smaller). If the required data to compute *EXF* are not available for the last sample year (2005), we use data for the last fiscal year ending before month *t*.

[11] We recognize that there is no economic interpretation for total prestige rankings when they are scaled by total assets. However, the scaling is intended to mitigate the size effect (large companies tend to have more analyst following) and is consistent with our measure of net external financing, *EXF*. We obtain similar results when *IB* is defined as the monthly quintile ranking of the number of analysts whose employers have investment banking businesses (i.e., the underwriter prestige ranking is greater than zero, regardless of the value of the ranking) divided by total assets.

$$REC_{it} = \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} + \beta_4 EXF_{it} + \beta_5 EXF_{it} * RULE_{it} + \beta_6 IB_{it} + \beta_7 IB_{it} * RULE_{it} + \varepsilon_{it}, \quad (7)$$

where all the variables are as defined previously. We are primarily interested in determining whether β_5 and β_7 in Equation (7) are significantly negative.

IV. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

Sample Selection and Data Sources

We collect analyst data from I/B/E/S, stock price and return data from CRSP, and financial statement data from Compustat. Our sample consists of all company-months with sufficient data during the period from 1994 to 2005. The sample period starts with 1994 because the number of companies with analyst recommendation data in I/B/E/S is much smaller before that year. We delete observations for which the longest-horizon earnings forecast or growth rate forecast is negative, as computation of the expected terminal value is problematic under these two situations. The full sample consists of 433,152 company-months,[12] with the number of companies in each month ranging from 2,399 to 3,675. The sample size decreases slightly in the analysis of V^{PEG}/P due to missing data on long-term growth rate forecasts (which are not required in the analysis of V^{RI}/P if the company has three- to five-year-ahead earnings forecasts).

Variable Definitions

As mentioned in footnote 8, we invert the coding of the stock recommendations in I/B/E/S so that 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. REC_t equals the consensus (mean) stock recommendation issued in month t.[13] Analysts' forecasts of earnings and long-term earnings growth rates equal the consensus (median) forecasts per I/B/E/S in the month. Earnings forecasted on a diluted basis are converted to a primary (or basic) basis based on the dilution factor provided by I/B/E/S that applies to the forecast. $BVPS_t$ equals total common equity (Compustat annual data #60) divided by common shares outstanding (#25). The cost of capital in month t equals the risk-free rate in the month plus the industry cost of equity (Fama and French 1997).[14] In the computation of V^{RI}/P and V^{PEG}/P, the earnings forecasts (stock prices) are adjusted for stock dividends and stock splits between the date of $BVPS_t$ and the date of the earnings forecasts (stock prices). Net external financing (*EXF*) in the regression for month t equals the sum of changes in the book value of common equity (#60), preferred stock (#130), short-term debt (#34), and long-term debt (#9) minus net income (#172) and then scaled by average total assets (#6), all for the first fiscal year ending after month t.

Descriptive Statistics of Variables

Table 1 shows the descriptive statistics of variables by year. The mean and median of *REC* decline in 2002 and continue to be lower during 2003–2005 than before. The mean

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[12] We also require that the earnings forecast for year $t+\tau$ (τ = 1,...,5) be issued within 15 months after the end of year t. This requirement results in 4,211 observations being deleted.

[13] We use the mean (instead of median) consensus recommendations because of the greater variation across companies.

[14] Our results are not sensitive to alternative discount rates used in computing V^{RI} (discussed in Section VI).

TABLE 1
Descriptive Statistics of Variables

Panel A: Means

Year	*REC*	V^{RI}/P	V^{PEG}/P	E/P	B/P	*LTG*	r	*EXF*	*IB*	n
1994	3.906	1.127	1.289	0.060	0.519	0.166	0.098	0.070	0.192	32,297
1995	3.925	0.856	1.297	0.064	0.498	0.170	0.116	0.089	0.193	34,017
1996	3.960	0.845	1.259	0.057	0.479	0.184	0.113	0.112	0.225	38,276
1997	4.012	0.809	1.221	0.049	0.438	0.195	0.113	0.111	0.208	42,217
1998	4.010	0.908	1.346	0.050	0.483	0.197	0.110	0.110	0.196	43,109
1999	3.985	0.988	1.383	0.056	0.553	0.193	0.107	0.102	0.189	40,655
2000	4.036	0.899	1.383	0.059	0.599	0.209	0.118	0.129	0.182	36,519
2001	3.960	1.023	1.195	0.042	0.612	0.199	0.103	0.070	0.124	30,775
2002	3.836	1.414	1.046	0.040	0.612	0.176	0.078	0.026	0.103	31,584
2003	3.579	1.561	0.931	0.041	0.554	0.156	0.071	0.053	0.091	33,774
2004	3.656	1.428	0.870	0.040	0.431	0.156	0.070	0.075	0.097	35,997
2005	3.650	0.986	0.873	0.038	0.435	0.155	0.087	0.105	0.097	33,932

Panel B: Medians

Year	*REC*	V^{RI}/P	V^{PEG}/P	E/P	B/P	*LTG*	r	*EXF*	*IB*	n
1994	4.000	0.944	1.148	0.063	0.463	0.150	0.099	0.016	0.076	32,297
1995	4.000	0.773	1.176	0.065	0.444	0.150	0.117	0.026	0.078	34,017
1996	4.000	0.754	1.106	0.060	0.424	0.150	0.114	0.024	0.082	38,276
1997	4.000	0.696	1.083	0.054	0.388	0.170	0.114	0.027	0.082	42,217
1998	4.000	0.735	1.127	0.054	0.400	0.175	0.112	0.031	0.083	43,109
1999	4.000	0.823	1.133	0.059	0.444	0.160	0.108	0.024	0.075	40,655
2000	4.000	0.764	1.144	0.061	0.426	0.170	0.120	0.017	0.069	36,519
2001	4.000	0.798	0.968	0.052	0.457	0.150	0.104	0.012	0.055	30,775
2002	3.860	1.055	0.932	0.051	0.487	0.150	0.081	0.002	0.046	31,584
2003	3.570	1.098	0.847	0.053	0.469	0.150	0.074	0.014	0.042	33,774
2004	3.670	0.984	0.805	0.049	0.397	0.150	0.073	0.014	0.043	35,997
2005	3.670	0.780	0.811	0.050	0.398	0.145	0.090	0.017	0.043	33,932

The statistics are based on company-months.

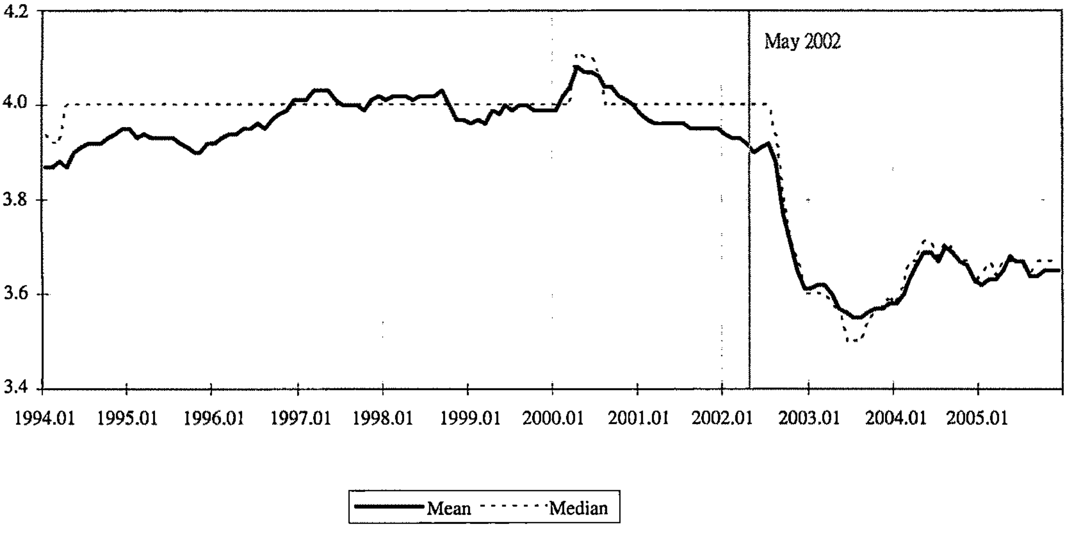
REC denotes analysts' consensus stock recommendation. Each individual analyst's recommendation is coded as follows: 1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy. V^{RI} is the intrinsic value of the stock, as defined in Equation (1) in the text. P is the closing stock price the day before the consensus recommendation date. V^{PEG} is the analysts' consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings (*LTG*). E/P is the consensus forecast of one-year-ahead earnings per share divided by P. B/P is the book value of common equity per share divided by P. r is the cost of capital, which equals the risk-free rate plus the industry cost of equity (Fama and French 1997). *EXF* equals the sum of changes in book value of common equity, preferred stock, short-term debt, and long-term debt minus net income and then scaled by the average total assets, all for the first fiscal year ending after the recommendation date. *IB* is the sum of the underwriter prestige rankings (based on Loughran and Ritter 2004) for the employers of the analysts issuing recommendations for the company's stock, divided by total assets (in million dollars) at the most recent fiscal year-end.

All variables in the table except *REC*, r, and *IB* are winsorized at the 1st and 99th percentiles of each month. The number of observations for V^{PEG}/P, *LTG*, *EXF*, and *IB* (not shown) are slightly lower due to missing data.

and median of V^{RI}/P (V^{PEG}/P) are higher (lower) in 2002–2004 than in other years.[15] Analysts' forecasts of one-year-ahead earnings per share (scaled by stock price) decline in 2001 but remain stable afterward. The results (not shown) are similar for analysts' forecasts of two-year-ahead earnings per share. The mean of analysts' forecasts of long-term growth rates declines in 2001–2003 but the median does not change during this period. The changes in book-to-market value of equity do not exhibit a clear pattern, while the cost of capital drops in 2002–2005 due to declines in the risk-free rate. *EXF* and *IB* are both lower after 2000, but untabulated statistics show that the declines are due to increases in total assets by a greater proportion than increases in the unscaled values of net external financing and total underwriter prestige rankings. Since we use monthly quintile rankings instead of raw values for the independent variables in the regressions, it is unlikely that the changes in the regression coefficients for *EXF* and *IB* are attributable to changes in the levels of these two variables over time.

Figure 1 shows a clear picture of changes in analyst stock recommendations around the implementation of Rule 2711. The mean recommendations decline gradually after March 2000 and drop dramatically in the second half of 2002. The median recommendations decline in the first half of 2000, remain stable afterward, and drop dramatically in the second half of 2002.

FIGURE 1
Means and Medians of Analysts' Stock Recommendations



This figure shows the mean and median of analysts' consensus stock recommendations for the full sample in each month during the 1994–2005 period. Stock recommendations are coded as follows: 1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy. The consensus stock recommendation for each company-month equals the mean of the individual analyst recommendations. There are 433,152 company-month observations in the full sample. The number of observations in each month ranges from 2,399 to 3,675.

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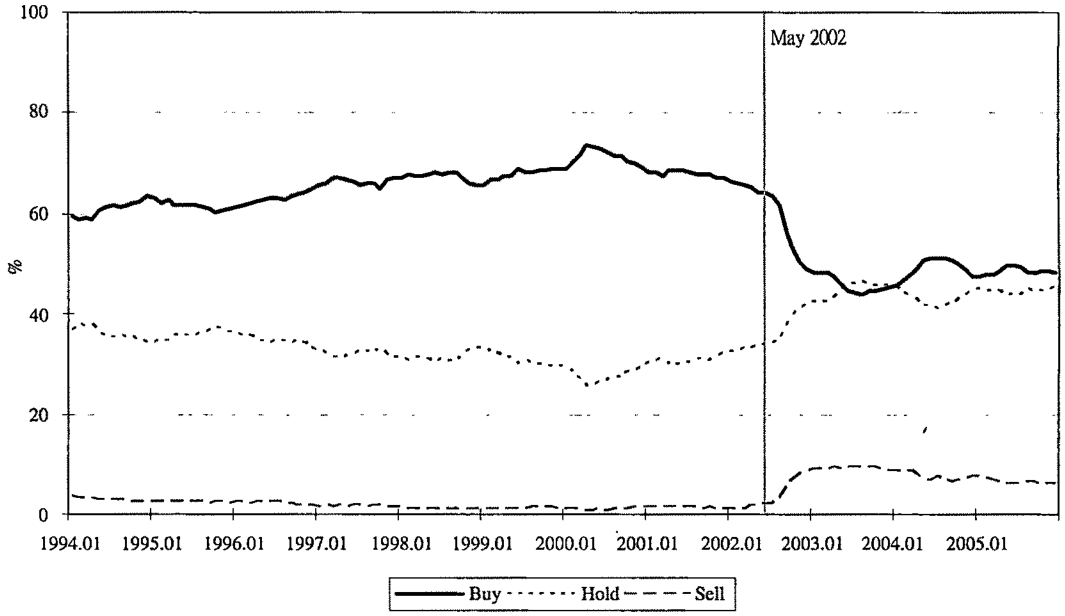
[15] Recall that we delete an observation if its longest-horizon earnings forecast is negative and that V^{PEG} is computed based on the forecast of two-year-ahead earnings per share. V^{PEG} could be negative if the longest forecast horizon exceeds two years and the two-year-ahead earnings forecast is negative.

To examine how specific changes in Buy, Hold, and Sell recommendations cause declines in *REC* after the implementation of Rule 2711, we examine the changes in the mean percentage of Buy, Hold, and Sell recommendations over time. Figure 2 shows that the percentage of analysts issuing Buy/Strong Buy recommendations decreases while that of analysts issuing Hold and Sell/Strong Sell recommendations both increase in the post-Rule period. These results are consistent with Barber et al. (2006) and reveal more balanced ratings distributions after Rule 2711 became effective.

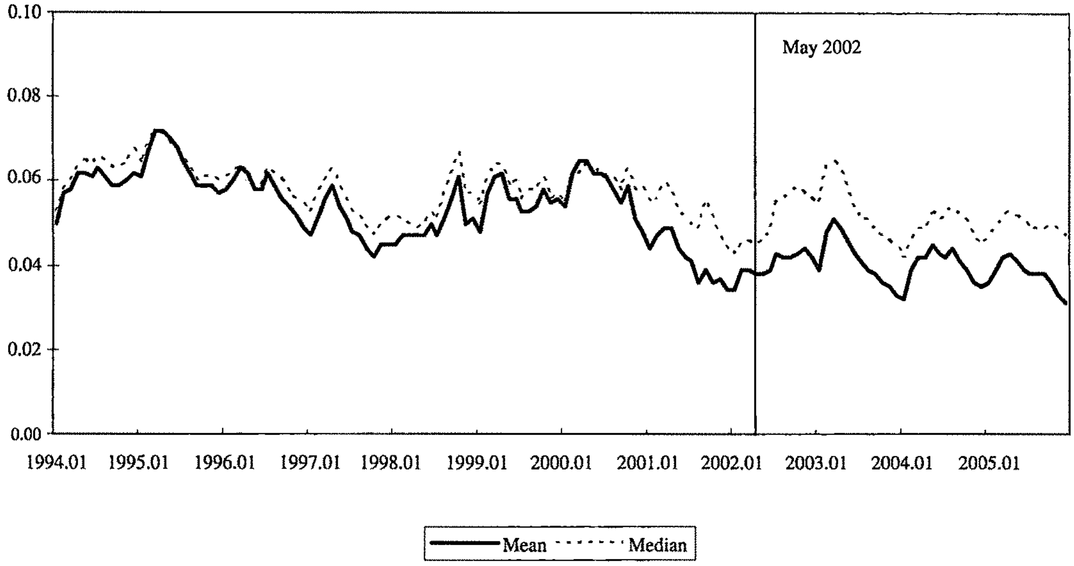
To check whether the big drops in stock recommendations during the second half of 2002 are driven by changes in companies' prospects, we examine the consensus forecasts of earnings (scaled by stock prices) and long-term growth rates in each month. Figure 3 shows that the mean and median analyst forecasts of one-year-ahead earnings per share scaled by stock prices decline in 2000 and 2001 but then increase in 2002. The statistics for analyst forecasts of two-year-ahead earnings (not shown) exhibit a similar pattern. Figure 4 shows that the mean analyst forecasts of the long-term growth rate gradually decline in 2002, but there is no change in the median forecasts during this period. Overall, the statistics in Figures 3 and 4 suggest that the significant drops in the mean and median stock recommendations around the implementation of Rule 2711 are more likely triggered by the Rule than by changes in companies' prospects.

FIGURE 2
Mean Percentages of Buy, Hold, and Sell Recommendations



This figure shows the mean percentages of analysts' Buy, Hold, and Sell recommendations for the full sample in each month during the 1994–2005 period, where the mean equals the sum of the percentages for all companies divided by the number of companies. In this figure, Strong Buy recommendations are included among Buy recommendations and Strong Sell recommendations are included among Sell recommendations.

FIGURE 3
Means and Medians of Analysts' Forecasts of One-Year-Ahead Earnings Per Share (Scaled by Stock Prices)



This figure shows the mean and median of analysts' consensus forecasts of one-year-ahead earnings per share for the full sample in each month during the 1994–2005 period. The consensus forecast for each company-month equals the median of the individual analyst forecasts scaled by the closing stock price the day before the consensus forecast date and winsorized at the 1st and 99th percentiles of each month.

V. EMPIRICAL RESULTS

Stock Recommendations and Intrinsic Value Estimates Relative to Stock Prices

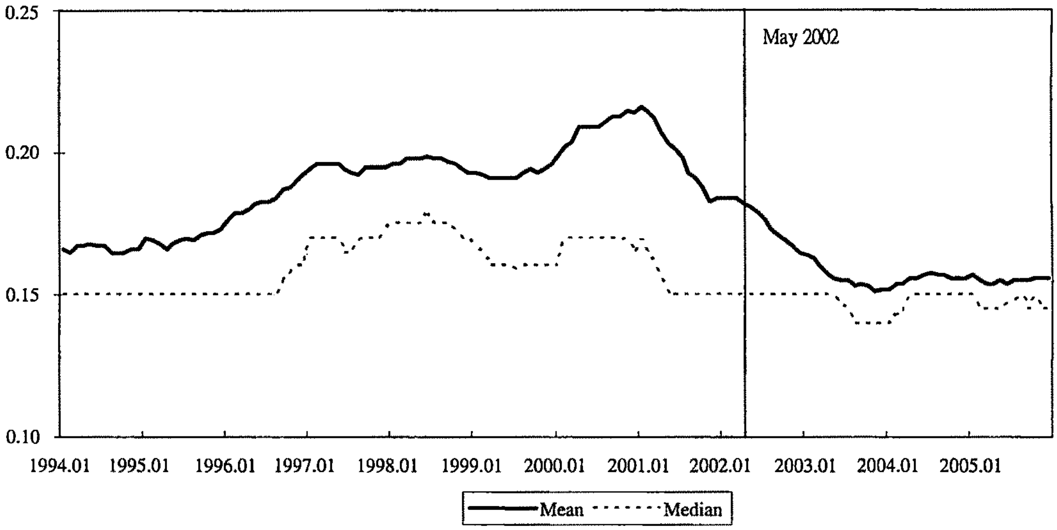
We perform monthly regressions of stock recommendations on V^{RI}/P and V^{PEG}/P (Equations (2) and (3)) and test if the means of the monthly coefficients for V^{RI}/P and V^{PEG}/P are significantly different between the pre-Rule period and the post-Rule period. In addition, we perform pooled regressions (Equation (4)) and test the statistical significance of the coefficients on $(V^{RI}/P) * RULE$ and $(V^{PEG}/P) * RULE$, where *RULE* equals 1 for post-Rule observations, and 0 otherwise.

Stock Recommendations and V^{RI}/P

The first two columns in Table 2, Panel A, show the means of the monthly coefficients on the intercept and V^{RI}/P for the pre-Rule period (100 months) and the post-Rule period (44 months), as well as the t-statistics of the mean coefficients. The mean coefficient on the intercept decreases significantly in the post-Rule period (difference = −0.435, t = −21.89), suggesting a decrease in analysts' average stock ratings after Rule 2711. The mean coefficient on V^{RI}/P for the pre-Rule period equals −0.024 (t = −1.90), consistent with the negative relation between analysts' stock recommendations and V^{RI}/P reported by Bradshaw (2004). However, the mean coefficient on V^{RI}/P equals 0.219 (t = 21.04) for the post-Rule period, which suggests that, on average, stocks in the highest quintile of V^{RI}/P receive a rating that is 0.219 higher than stocks in the lowest quintile of V^{RI}/P after Rule 2711 was implemented. This gap is not trivial compared with the mean *REC* in 2003–2005,

FIGURE 4

Means and Medians of Analysts' Forecasts of Long-Term Earnings Growth Rates



This figure shows the mean and median of analysts' consensus forecasts of long-term earnings growth rates for the full sample in each month during the 1994–2005 period. The consensus forecast for each company-month equals the median of the individual analyst forecasts.

which ranges between 3.579 and 3.656 (shown in Table 1, Panel A). The mean coefficient on V^{RI}/P for the post-Rule period is significantly higher than that for the pre-Rule period (difference = 0.244, t = 14.73).

The first two columns in Table 2, Panel B, show the results for the pooled regression of Equation (4).[16] The coefficient on *RULE* equals −0.441 (t = −36.04), suggesting an overall decrease in analysts' stock recommendations after Rule 2711. This coefficient is close to the difference in the mean coefficients on the intercept between the pre- and post-Rule periods (−0.435) reported in Panel A. The coefficient on V^{RI}/P equals −0.023, which is close to the mean coefficient on V^{RI}/P for the pre-Rule period (−0.024) reported in Panel A. The coefficient on $(V^{RI}/P) * RULE$ equals 0.244, which is identical to the difference in the mean coefficients on V^{RI}/P between the two time periods reported in Panel A. Finally, the sum of the coefficients on V^{RI}/P and $(V^{RI}/P) * RULE$ equals 0.221, again close to the mean coefficient on V^{RI}/P for the post-Rule period (0.219) reported in Panel A. Taken together, the results in Table 2 for V^{RI}/P are consistent with our first hypothesis that analysts' stock recommendations are more strongly related to the intrinsic value estimates relative to the stock prices after Rule 2711 than before the Rule was implemented.

Stock Recommendations and V^{PEG}/P

The third and fourth columns in Table 2, Panel A, show the means and t-statistics of the means of the monthly coefficients on the intercept and V^{PEG}/P. The mean coefficient

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[16] In all the pooled regressions, we compute the t-statistics using robust standard errors adjusted for clustering by company. In other words, we treat the observations as independent across companies but not necessarily independent within the same company.

TABLE 2
Summary Results from Regression of Analysts' Stock Recommendations on Valuation Estimates

Panel A: Mean Coefficients from Monthly Regression

$$REC_{it} = \beta_0 + \beta_1 VALUATION_{it} + \varepsilon_{it}$$

(100 and 44 months for the pre-Rule period and post-Rule period, respectively)

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
	Mean Coefficient	**t-statistic**	**Mean Coefficient**	**t-statistic**
Results for β_0 (Intercept)				
Pre-Rule 2711	3.984	426.95***	3.751	293.81***
Post-Rule 2711	3.549	202.12***	3.385	202.35***
Difference	−0.435	−21.89***	−0.365	−16.43***
Results for β_1 (*VALUATION*)				
Pre-Rule 2711	−0.024	−1.90*	0.440	22.08***
Post-Rule 2711	0.219	21.04***	0.536	65.34***
Difference	0.244	14.73***	0.096	4.45***

Panel B: Pooled Regression

$$REC_{it} = \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} + \varepsilon_{it}$$

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	3.987	490.52***	3.753	455.13***
RULE	−0.441	−36.04***	−0.369	−29.79***
VALUATION	−0.023	−1.87*	0.443	38.11***
*VALUATION * RULE*	0.244	12.70***	0.093	4.99***
$\beta_2 + \beta_3$	0.221	13.05***	0.536	33.48***
Adjusted R^2	0.062		0.135	
Number of observations	433,152		428,170	

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*, *** Significant at 10 percent and 1 percent levels, respectively (two-tailed).

REC_{it} denotes analysts' consensus stock recommendation for company *i* issued in month *t* (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). The pre-Rule period is from January 1, 1994 to May 9, 2002 and the post-Rule period is from May 10, 2002 to December 31, 2005. $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company *i* estimated in month *t* (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month *t*. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company *i* issued in month *t*, and divided by the closing stock price the day before the recommendation date in month *t*. In the regressions, V^{RI}/P and V^{PEG}/P are replaced by the monthly quintile ranking scaled to range between 0 and 1. The sample consists of all observations with sufficient data in the 1994–2005 period.

Panel A shows the mean coefficients for the monthly regressions for the pre- and post-Rule periods, t-statistics computed based on the standard errors of the monthly coefficients, and results from t-tests for difference in the mean coefficients between the pre- and post-Rule periods.

Panel B shows the results from pooled regression, where $RULE_{it}$ is an indicator variable that equals 1 if the observation is from the post-Rule 2711 period, and 0 otherwise; the t-statistics are computed using robust standard errors adjusted for clustering by company.

on the intercept decreases significantly in the post-Rule period (difference = −0.365, t = −16.43), consistent with the result in the first two columns. The mean coefficient on V^{PEG}/P for the pre-Rule period equals 0.440 (t = 22.08), consistent with Bradshaw (2004). The mean coefficient on V^{PEG}/P for the post-Rule period equals 0.536 (t = 65.34), and the difference in the mean coefficient on V^{PEG}/P between the two time periods equals 0.096 (t = 4.45).

The third and fourth columns in Table 2, Panel B, show that in the pooled regression, the coefficient on *RULE* equals −0.369 (t = −29.79), which is close to the difference in the mean coefficients on the intercept between the pre-Rule period and the post-Rule period (−0.365) shown in Panel A. The coefficient on V^{PEG}/P equals 0.443 (t = 38.11), the coefficient on $(V^{PEG}/P) * RULE$ equals 0.093 (t = 4.99), and the sum of these two coefficients equals 0.536 (t = 33.48), all close to the means of the monthly coefficients shown in Panel A. Overall, the results in Table 2 also show that analysts' stock recommendations are more strongly related to V^{PEG}/P after Rule 2711 than before the Rule was implemented, consistent with our first hypothesis.

Stock Recommendations and Conflicts of Interest

Table 3 presents the results from regressions of stock recommendations on the company's net external financing (*EXF*) and the underwriter prestige rankings of the analysts' employers (*IB*) while controlling for V^{RI}/P and V^{PEG}/P. Panel A shows that, for the monthly regression (Equations (5) and (6)), the mean coefficient on V^{RI}/P for the pre-Rule period is no longer significantly negative when the proxies for conflicts of interest are included in the regression, but the mean coefficient on V^{RI}/P for the post-Rule period remains significantly positive. Panel A also shows that the mean coefficient on V^{PEG}/P is significantly positive for both time periods, but it is significantly higher in the post-Rule period, consistent with the results in Table 2. Regarding the proxies for conflicts of interest, the first two columns in Table 3, Panel A, show that the mean coefficient on *EXF* equals 0.340 and 0.242 for the pre-Rule period and the post-Rule period, respectively, a nearly 30 percent decrease after Rule 2711. The mean coefficient on *IB* decreases from 0.253 for the pre-Rule period to 0.138 for the post-Rule period, a 45 percent decrease. The third and fourth columns in Panel A also show statistically significant decreases in the mean coefficients for *EXF* and *IB* in the post-Rule period when the valuation estimate equals V^{PEG}/P.

Panel B of Table 3 shows the results for the pooled regression of Equation (7). In the first two columns, the coefficient on V^{RI}/P is no longer significantly negative after controlling for conflicts of interest, but the coefficient on $(V^{RI}/P) * RULE$ remains significantly positive. The coefficient on *EXF* equals 0.349 (t = 33.38), the coefficient on *EXF* * *RULE* equals −0.105 (t = −5.52), and the sum of these two coefficients equals 0.244 (t = 14.64). These values are close to the means of the monthly coefficients for *EXF* shown in Panel A. The coefficient on *IB* equals 0.262 (t = 19.18), the coefficient on *IB* * *RULE* equals −0.126 (t = −5.87), and the sum of these two coefficients equals 0.136 (t = 7.09). These values also are close to the means of the monthly coefficients for *IB* shown in Panel A. Similar regression results are found for the two proxies for conflicts of interest when V^{RI}/P is replaced by V^{PEG}/P, as shown in the third and fourth columns in Panel B.

Taken together, the results in Table 3 demonstrate that analysts' stock recommendations are positively related to the company's external financing activities and the amount of underwriting business provided by the analysts' employers. The positive relations persist but become significantly weaker in the post-Rule period. These results suggest that the

TABLE 3
Summary Results from Regression of Analysts' Stock Recommendations on Valuation Estimates, External Financing, and Investment Banking Services

Panel A: Mean Coefficients from Monthly Regression

$$REC_{it} = \beta_0 + \beta_1 VALUATION_{it} + \beta_2 EXF_{it} + \beta_3 IB_{it} + \varepsilon_{it}$$

(100 and 44 months for the pre-Rule period and post-Rule period, respectively)

	VALUATION = V^{RI}/P		*VALUATION* = V^{PEG}/P	
	Mean Coefficient	**t-statistic**	**Mean Coefficient**	**t-statistic**
Results for β_0 (Intercept)				
Pre-Rule 2711	3.688	346.59***	3.565	410.44***
Post-Rule 2711	3.363	201.87***	3.268	198.45***
Difference	−0.325	−16.71***	−0.297	−15.96***
Results for β_1 (*VALUATION*)				
Pre-Rule 2711	−0.012	−1.21	0.365	23.98***
Post-Rule 2711	0.216	20.35***	0.506	62.79***
Difference	0.228	15.76***	0.141	8.16***
Results for β_2 (*EXF*)				
Pre-Rule 2711	0.340	64.64***	0.314	61.77***
Post-Rule 2711	0.242	37.92***	0.192	38.30***
Difference	−0.098	−10.85***	−0.121	−16.99***
Results for β_3 (*IB*)				
Pre-Rule 2711	0.253	36.18***	0.146	22.65***
Post-Rule 2711	0.138	16.33***	0.082	9.36***
Difference	−0.115	−9.66***	−0.064	−5.66***

Panel B: Pooled Regression

$$REC_{it} = \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} + \beta_4 EXF_{it} + \beta_5 EXF_{it} * RULE_{it} + \beta_6 IB_{it} + \beta_7 IB_{it} * RULE_{it} + \varepsilon_{it}$$

	VALUATION = V^{RI}/P		*VALUATION* = V^{PEG}/P	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	3.680	320.42***	3.554	343.58***
RULE	−0.319	−18.01***	−0.288	−17.18***
VALUATION	−0.006	−0.55	0.356	31.68***
*VALUATION * RULE*	0.222	11.77***	0.150	7.98***
EXF	0.349	33.38***	0.334	33.37***
*EXF * RULE*	−0.105	−5.52***	−0.140	−7.69***
IB	0.262	19.18***	0.165	12.61***
*IB * RULE*	−0.126	−5.87***	−0.084	−3.99***
$\beta_2 + \beta_3$	0.216	12.86***	0.506	31.22***
$\beta_4 + \beta_5$	0.244	14.64***	0.194	12.28***
$\beta_6 + \beta_7$	0.136	7.09***	0.081	4.44***
Adjusted R^2	0.134		0.182	
Number of observations	411,645		406,794	

(*continued on next page*)

TABLE 3 (continued)

*** Significant at 1 percent level (two-tailed).

REC_{it} denotes analysts' consensus stock recommendation for company i issued in month t (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). The pre-Rule period is from January 1, 1994 to May 9, 2002 and the post-Rule period is from May 10, 2002 to December 31, 2005. $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company i estimated in month t (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month t. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company i issued in month t, and divided by the closing stock price the day before the recommendation date in month t. EXF_{it} equals the sum of changes in book value of common equity, preferred stock, short-term debt, and long-term debt minus net income and then divided by average total assets, all for the first fiscal year ending after month t. IB_{it} is the sum of the underwriter prestige rankings (based on Loughran and Ritter 2004) for the employers of the analysts who have issued recommendations for company i stock by month t, divided by total assets (in million dollars) at the last fiscal year-end before month t. In the regressions, all the independent variables are replaced by the monthly quintile ranking scaled to range between 0 and 1. The sample consists of all observations with sufficient data in the 1994–2005 period.

Panel A shows the mean coefficients for the monthly regressions for the pre- and post-Rule periods, t-statistics computed based on the standard errors of the monthly coefficients, and results from t-tests for difference in the mean coefficients between the pre- and post-Rule periods.

Panel B shows the results from pooled regression, where $RULE_{it}$ is an indicator variable that equals 1 if the observation is from the post-Rule 2711 period, and 0 otherwise; the t-statistics are computed using robust standard errors adjusted for clustering by company.

influence of conflicts of interest on analysts' stock recommendations has weakened since the introduction of Rule 2711, consistent with our second hypothesis.[17]

The Relation between Stock Recommendations and Valuation Estimates Conditional on the Level of Potential Conflicts of Interest

To enhance the likelihood that our findings of stronger relations between analysts' stock recommendations and the intrinsic value estimates relative to the stock prices in the post-Rule period are attributable to the implementation of Rule 2711, we estimate the following regression:

$$\begin{aligned} REC_{it} = {} & \beta_0 + \beta_1 VALUATION_{it} + \beta_2 HIEXFIB_{it} \\ & + \beta_3 HIEXFIB_{it} * VALUATION_{it} + \varepsilon_{it}, \end{aligned} \tag{8}$$

where $HIEXFIB_{it}$ is an indicator variable that equals 1 if both EXF_{it} and IB_{it} are above the median of month t, and 0 otherwise; other variables are as defined previously. We estimate Equation (8) by month to see if the increases in the relation between stock recommendations and V^{RI}/P and V^{PEG}/P are greater for the observations followed by analysts who are likely to be more affected by Rule 2711 (i.e., with greater potential conflicts of interest induced by investment banking opportunities). Therefore, we are interested in finding whether β_3 changes in the post-Rule period.

Table 4, Panel A shows the means of the monthly coefficients in Equation (8). Regardless of whether *VALUATION* equals V^{RI}/P or V^{PEG}/P, the mean coefficient on *HIEXFIB* is positive for both time periods. However, it is significantly lower for the post-Rule period,

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[17] A positive relation between analysts' stock recommendations and net external financing is also consistent with managers timing the company's financing activities with analyst optimism (Kolasinski and Kothari 2007; Bradshaw et al. 2006). However, since the average stock recommendations declined in every month during the period around the implementation of Rule 2711, the above explanation would be plausible only if the manager could start and complete a financing activity within a very short period of time (e.g., one month).

TABLE 4

Summary Results from Regression of Analysts' Stock Recommendations on Valuation Estimates, a Dummy Variable Representing Greater External Financing and Greater Investment Banking Services, and the Interaction between the Dummy Variable and Valuation Estimates

Panel A: Mean Coefficients from Monthly Regression

$$REC_{it} = \beta_0 + \beta_1 VALUATION_{it} + \beta_2 HIEXFIB_{it} + \beta_3 HIEXFIB_{it} * VALUATION_{it} + \varepsilon_{it}$$

(100 and 44 months for the pre-Rule period and post-Rule period, respectively)

	VALUATION = V^{RI}/P		*VALUATION* = V^{PEG}/P	
	Mean Coefficient	**t-statistic**	**Mean Coefficient**	**t-statistic**
Results for β_0 (Intercept)				
Pre-Rule 2711	3.885	451.69***	3.670	369.91***
Post-Rule 2711	3.504	199.82***	3.338	203.46***
Difference	−0.381	−19.49***	−0.332	−17.95***
Results for β_1 (*VALUATION*)				
Pre-Rule 2711	0.003	0.29	0.474	29.74***
Post-Rule 2711	0.206	17.17***	0.558	72.23***
Difference	0.203	11.95***	0.084	4.73***
Results for β_2 (*HIEXFIB*)				
Pre-Rule 2711	0.319	44.91***	0.382	39.57***
Post-Rule 2711	0.171	23.56***	0.216	24.52***
Difference	−0.148	−14.52***	−0.166	−12.73***
Results for β_3 (*HIEXFIB* * *VALUATION*)				
Pre-Rule 2711	−0.043	−6.48***	−0.265	−26.70***
Post-Rule 2711	0.032	3.15***	−0.139	−13.72***
Difference	0.075	6.23***	0.127	8.94***

Panel B: Pooled Regression

$$\begin{aligned} REC_{it} = {} & \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} + \beta_4 HIEXFIB_{it} \\ & + \beta_5 HIEXFIB_{it} * RULE_{it} + \beta_6 HIEXFIB_{it} * VALUATION_{it} \\ & + \beta_7 HIEXFIB_{it} * VALUATION_{it} * RULE_{it} + \varepsilon_{it} \end{aligned}$$

	VALUATION = V^{RI}/P		*VALUATION* = V^{PEG}/P	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	3.883	376.32***	3.666	392.40***
RULE	−0.379	−25.42***	−0.327	−22.83***
VALUATION	0.009	0.55	0.481	33.79***
VALUATION * *RULE*	0.196	8.07***	0.073	3.19***
HIEXFIB	0.332	25.49***	0.422	29.83***
HIEXFIB * *RULE*	−0.164	−7.42***	−0.214	−8.71***
HIEXFIB * *VALUATION*	−0.049	−2.44**	−0.302	−15.22***
HIEXFIB * *VALUATION* * *RULE*	0.087	2.53**	0.178	5.03***
$\beta_2 + \beta_3$	0.204	9.47***	0.554	28.18***
$\beta_4 + \beta_5$	0.167	8.50***	0.208	9.76***
$\beta_6 + \beta_7$	0.039	1.28	−0.124	−4.04***
Adjusted R^2	0.110		0.172	
Number of observations	411,645		406,794	

(continued on next page)

TABLE 4 (continued)

, * Significant at 5 percent and 1 percent levels, respectively (two-tailed).

REC_{it} denotes analysts' consensus stock recommendation for company i issued in month t (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). The pre-Rule period is from January 1, 1994 to May 9, 2002 and the post-Rule period is from May 10, 2002 to December 31, 2005. $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company i estimated in month t (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month t. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company i issued in month t, and divided by the closing stock price the day before the recommendation date in month t. EXF_{it} equals the sum of changes in book value of common equity, preferred stock, short-term debt, and long-term debt, minus net income and then divided by average total assets, all for the first fiscal year ending after month t. IB_{it} is the sum of the underwriter prestige rankings (based on Loughran and Ritter 2004) for the employers of the analysts who have issued recommendations for company i stock by month t, divided by total assets (in million dollars) at the last fiscal year-end before month t. $HIEXFIB_{it}$ is an indicator variable, equal to 1 if both EXF_{it} and IB_{it} are above-median in month t, and 0 otherwise. In the regressions, V^{RI}/P and V^{PEG}/P are replaced by the monthly quintile ranking scaled to range between 0 and 1. The sample consists of all observations with sufficient data in the 1994–2005 period.

Panel A shows the mean coefficients for the monthly regressions for the pre- and post-Rule periods, t-statistics computed based on the standard errors of the monthly coefficients, and results from t-tests for difference in the mean coefficients between the pre- and post-Rule periods.

Panel B shows the results from pooled regression, where $RULE_{it}$ is an indicator variable that equals 1 if the observation is from the post-Rule 2711 period, and 0 otherwise; the t-statistics are computed using robust standard errors adjusted for clustering by company.

indicating that the higher ratings of stocks with both high *EXF* and high *IB* is not as evident after Rule 2711 as before the Rule. The mean coefficient on *HIEXFIB * VALUATION* is significantly negative for the pre-Rule period, and it is significantly positive (negative) for the post-Rule period when *VALUATION* equals V^{RI}/P (V^{PEG}/P). More importantly, the mean coefficient on *HIEXFIB * VALUATION* for the post-Rule period is significantly higher than that for the pre-Rule period, which indicates that analysts' recommendations for stocks with both high *EXF* and high *IB* observe significantly greater changes in the relation with value estimates relative to the stock prices in the post-Rule period compared with the recommendations for other stocks.

In addition to the monthly regressions for Equation (8), we estimate the following pooled regression, with a focus on the results for β_7:

$$\begin{aligned} REC_{it} = {} & \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} \\ & + \beta_4 HIEXFIB_{it} + \beta_5 HIEXFIB_{it} * RULE_{it} + \beta_6 HIEXFIB_{it} * VALUATION_{it} \\ & + \beta_7 HIEXFIB_{it} * VALUATION_{it} * RULE_{it} + \varepsilon_{it}. \qquad (9) \end{aligned}$$

Table 4, Panel B shows the coefficients from the regression of Equation (9). Regardless of the valuation estimate used, the coefficient on *HIEXFIB * RULE* is significantly negative, consistent with the results in Panel A that show different mean coefficients on *HIEXFIB* between the pre- and post-Rule periods. Panel B also shows that the coefficient on *HIEXFIB * VALUATION * RULE* is significantly positive, again, regardless of whether *VALUATION* equals V^{RI}/P or V^{PEG}/P. These results are consistent with those from a test of the difference in the coefficient on *HIEXFIB * VALUATION* between the two time periods shown in Panel A. Overall, the results in Table 4 suggest that, prior to Rule 2711, analysts recommending stocks with both higher *EXF* and higher *IB* incorporate V^{RI}/P and V^{PEG}/P into their stock recommendations to a lesser extent than do other analysts. After the Rule, analysts recommending stocks with both higher *EXF* and higher *IB* increase their incorporation of V^{RI}/P and V^{PEG}/P into their stock recommendations by a greater extent than do the other analysts.

Since the stock recommendations for companies that have higher levels of external financing activities and are covered by analysts whose employers engage in larger amounts of underwriting business are more likely to be impacted by Rule 2711, the results in Table 4 provide additional evidence that links changes in the relation between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P with the implementation of Rule 2711.

VI. ADDITIONAL ANALYSES

In this section, we examine whether our findings of changes in the relation between stock recommendations and value estimates relative to stock prices can be attributed to alternative factors or regulatory events unrelated to Rule 2711. In addition, we examine whether our findings are robust to alternative specifications or parameters used in the valuation models. Last, we examine analysts' potential conflict of interest arising from commission revenues as well as the profitability of analyst stock recommendations before and after the implementation of Rule 2711.

Regulation Fair Disclosure (Reg FD)

Reg FD (implemented on October 23, 2000) prohibits managers from privately disclosing material information to financial analysts and thus could affect the information environment faced by analysts. To examine whether our results are attributable to Reg FD, we divide the pre-Rule period into pre-FD and post-FD periods using the implementation date of Reg FD as a cut-off and then replicate the regression analyses. The results are presented in Table 5. In both panels, there is no evidence that the coefficient on either V^{RI}/P or V^{PEG}/P is significantly higher in the post-FD but pre-Rule 2711 period than in the pre-FD period. Instead, there is evidence that the coefficients on V^{RI}/P and V^{PEG}/P decrease in the post-FD but pre-Rule 2711 period and then increase significantly after the implementation of Rule 2711.

In addition, if Reg FD affects analysts' incorporation of V^{RI}/P and V^{PEG}/P into stock recommendations, there should be different effects between the companies affected more by Reg FD (i.e., a higher likelihood of private communications between managers and analysts in the pre-FD period) and those affected less by Reg FD. We use the accuracy and dispersion of analysts' earnings forecasts as proxies for the likelihood of private communications between managers and analysts (Agrawal et al. 2006; Mohanram and Sunder 2006). Observations with above-median forecast accuracy or below-median forecast dispersion (or both) in the month are classified as being affected more by Reg FD. We find that these two groups of companies do not have significant differences in the relations between stock recommendations and V^{RI}/P and V^{PEG}/P during the post-FD period (results not shown). These findings suggest that it is unlikely that the strong relation between analysts' stock recommendations and value estimates relative to stock prices observed in recent years is caused by the implementation of Reg FD.

Analysts' Substitution of Investment Signals

One possible explanation for the changes in the relation of stock recommendations with V^{RI}/P and V^{PEG}/P is that analysts put different weights on various investment signals depending on the prevailing market conditions. Prior research has found that analysts' stock recommendations are related to price momentum (e.g., Jegadeesh et al. 2004). To test if the changes in the regression coefficients on V^{RI}/P and V^{PEG}/P are caused by analysts substituting price momentum with V^{RI}/P and V^{PEG}/P in the post-Rule period, we include

TABLE 5
Summary Results from Regression of Analysts' Stock Recommendations on Valuation Estimates
(Three Subperiods: Pre-Reg FD, Post-Reg FD but Pre-Rule 2711, and Post-Rule 2711)

Panel A: Mean Coefficients from Monthly Regression

$$REC_{it} = \beta_0 + \beta_1 VALUATION_{it} + \varepsilon_{it}$$

(82, 18, and 44 months for the pre-Reg FD, post-Reg FD but pre-Rule 2711, and post-Rule 2711 period, respectively)

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
	Mean Coefficient	t-statistic	Mean Coefficient	t-statistic
Results for β_0 (Intercept)				
(1) Pre-Reg FD	3.983	381.75***	3.737	253.65***
(2) Post-Reg FD but Pre-Rule 2711	3.990	187.66***	3.814	230.56***
(3) Post-Rule 2711	3.549	202.12***	3.385	202.35***
Difference: (2) − (1)	0.007	0.29	0.077	3.49***
Difference: (3) − (2)	−0.441	−14.38***	−0.429	−18.21***
Results for β_1 (*VALUATION*)				
(1) Pre-Reg FD	−0.015	−1.11	0.474	21.52***
(2) Post-Reg FD but Pre-Rule 2711	−0.065	−2.02**	0.283	12.23***
(3) Post-Rule 2711	0.219	21.04***	0.536	65.34***
Difference: (2) − (1)	−0.050	−1.50	−0.191	−5.98***
Difference: (3) − (2)	0.284	8.39***	0.253	10.29***

Panel B: Pooled Regression

$$REC_{it} = \beta_0 + \beta_1 FD_{it} + \beta_2 RULE_{it} + \beta_3 VALUATION_{it} + \beta_4 VALUATION_{it} * FD_{it} + \beta_5 VALUATION_{it} * RULE_{it} + \varepsilon_{it}$$

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
Variable	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	3.986	453.54***	3.741	425.33***
FD	0.008	0.65	0.075	6.07***
RULE	−0.439	−33.97***	−0.357	−27.47***
VALUATION	−0.014	−1.08	0.472	37.78***
*VALUATION * FD*	−0.057	−2.72***	−0.193	−9.73***
*VALUATION * RULE*	0.235	11.61***	0.064	3.27***
$\beta_5 - \beta_4$	0.292	12.99***	0.257	11.61***
Adjusted R^2	0.063		0.136	
Number of observations	433,152		428,170	

, * Significant at 5 percent and 1 percent levels, respectively (two-tailed).

REC_{it} denotes analysts' consensus stock recommendation for company *i* issued in month *t* (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). The pre-Reg FD period is from January 1, 1994 to October 22, 2000, the post-Reg FD but pre-Rule 2711 period is from October 23, 2000 to May 9, 2002, and the post-Rule 2711 period is from May 10, 2002 to December 31, 2005. $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company *i* estimated in month *t* (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month *t*. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company *i* issued in month *t*, and divided by the closing stock price the day before the recommendation date in month *t*. In the regressions, V^{RI}/P and V^{PEG}/P are replaced by the monthly quintile ranking scaled to range between 0 and

(*continued on next page*)

TABLE 5 (continued)

1. The sample consists of all observations with sufficient data in 1994–2005.

Panel A shows the mean coefficients for the monthly regressions for the three subperiods, t-statistics computed based on the standard errors of the monthly coefficients, and results from t-tests for difference in the mean coefficients between any two time periods.

Panel B shows the results from pooled regression, where FD_{it} is an indicator variable that equals 1 if the observation is from the post-Reg FD but pre-Rule 2711 period, and 0 otherwise; $RULE_{it}$ is an indicator variable that equals 1 if the observation is from the post-Rule 2711 period, and 0 otherwise; the t-statistics are computed using robust standard errors adjusted for clustering by company.

in the regressions a proxy variable for price momentum that is equal to the stock's cumulative abnormal return over the month before the consensus recommendation date (transformed into the monthly quintile ranking and scaled to range between 0 and 1).[18] The results are presented in Table 6. The mean coefficient on the lagged abnormal return and the coefficient on the lagged abnormal return in the pooled regression are significantly positive both before and after Rule 2711, consistent with price momentum explaining stock recommendations. The results for V^{RI}/P and V^{PEG}/P from testing the mean coefficients and running the pooled regressions are similar to those reported in Table 2. When we replace the lagged one-month abnormal returns with lagged six-month abnormal returns, the regression results (not tabulated) again are similar.

Analysts' Use of Fundamental Valuation Conditional on Market Sentiment

Another possible explanation for our findings relates to market sentiment. If analysts are more (less) inclined to rely on fundamentals when market sentiment is weaker (stronger), then the change in the coefficients on V^{RI}/P and V^{PEG}/P may simply reflect the change in the overall market level. To examine this alternative explanation, we check whether the monthly coefficients for V^{RI}/P and V^{PEG}/P are negatively correlated with the return from the S&P 500 Index over the month before the consensus recommendation date. We find that the Pearson correlation coefficient for V^{RI}/P equals -0.080 ($p = 0.428$) in the pre-Rule period and 0.362 ($p = 0.016$) in the post-Rule period and that the Pearson correlation coefficient for V^{PEG}/P equals 0.170 ($p = 0.092$) in the pre-Rule period and 0.381 ($p = 0.011$) in the post-Rule period.

We also regress the monthly coefficients for V^{RI}/P and V^{PEG}/P (obtained from Equation (2)) on the return from the S&P 500 Index over the month before the consensus recommendation date. The results are presented in Table 7. The monthly coefficient for V^{RI}/P is not significantly associated with the return from the S&P 500 Index and the monthly coefficient for V^{PEG}/P is significantly positively associated with the return from the S&P 500 Index in the pre-Rule period. The coefficient on the interaction between the return from the S&P 500 Index and *RULE* is insignificant, suggesting that the influence of market movement on the monthly coefficients of V^{RI}/P and V^{PEG}/P does not change after Rule 2711. The coefficient on *RULE* is significantly positive, suggesting an increase in the relations of stock recommendations with V^{RI}/P and V^{PEG}/P in the post-Rule period, after controlling for the market movement. Taken together, the results in Table 7 provide no

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[18] We do not examine the other fundamental signals examined by Jegadeesh et al. (2004) because those signals are related to accounting information and are likely to be incorporated into analysts' earnings forecasts, an input to our intrinsic value estimates.

TABLE 6
Summary Results from Regression of Analysts' Stock Recommendations on Valuation Estimates and Lagged Abnormal Returns

Panel A: Mean Coefficients from Monthly Regression

$$REC_{it} = \beta_0 + \beta_1 VALUATION_{it} + \beta_2 LAR_{it} + \varepsilon_{it}$$

(100 and 44 months for the pre-Rule period and post-Rule period, respectively)

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
	Mean Coefficient	**t-statistic**	**Mean Coefficient**	**t-statistic**
Results for β_0 (Intercept)				
Pre-Rule 2711	3.933	312.01***	3.664	236.53***
Post-Rule 2711	3.476	177.46***	3.303	195.26***
Difference	−0.457	−19.87***	−0.361	−15.76***
Results for β_1 (*VALUATION*)				
Pre-Rule 2711	−0.007	−0.56	0.461	23.11***
Post-Rule 2711	0.234	23.87***	0.548	76.23***
Difference	0.241	14.90***	0.087	4.07***
Results for β_2 (*LAR*)				
Pre-Rule 2711	0.085	6.69***	0.151	13.49***
Post-Rule 2711	0.132	9.35***	0.153	12.92***
Difference	0.047	2.47**	0.002	0.11

Panel B: Pooled Regression

$$REC_{it} = \beta_0 + \beta_1 RULE_{it} + \beta_2 VALUATION_{it} + \beta_3 VALUATION_{it} * RULE_{it} + \beta_4 LAR_{it} + \beta_5 LAR_{it} * RULE_{it} + \varepsilon_{it}$$

	$VALUATION = V^{RI}/P$		$VALUATION = V^{PEG}/P$	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	3.937	438.00***	3.665	408.78***
RULE	−0.463	−34.58***	−0.364	−26.95***
VALUATION	−0.007	−0.57	0.465	39.75***
*VALUATION * RULE*	0.242	12.48***	0.084	4.45***
LAR	0.085	24.19***	0.154	45.62***
*LAR * RULE*	0.048	7.88***	−0.000	−0.01
$\beta_2 + \beta_3$	0.235	13.77***	0.549	34.14***
$\beta_4 + \beta_5$	0.133	25.85***	0.154	30.87***
Adjusted R^2	0.066		0.143	
Number of observations	432,859		427,889	

, * Significant at 5 percent and 1 percent levels, respectively (two-tailed).

REC_{it} denotes analysts' consensus stock recommendation for company i issued in month t (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). The pre-Rule period is from January 1, 1994 to May 9, 2002 and the post-Rule period is from May 10, 2002 to December 31, 2005. $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company i estimated in month t (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month t. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company i issued in month t, and divided by the closing stock price the day before the consensus recommendation date in month t. LAR_{it} equals the stock's lagged abnormal return, cumulated over the month before the consensus recommendation date in month t. In the regressions, all the independent variables are replaced by the monthly quintile ranking scaled to range between 0 and 1. The sample consists of all observations with sufficient data in the 1994–2005 period.

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TABLE 6 (continued)

Panel A shows the mean coefficients for the monthly regressions for the pre- and post-Rule periods, t-statistics computed based on the standard errors of the monthly coefficients, and results from t-tests for difference in the mean coefficients between the pre- and post-Rule periods.

Panel B shows the results from pooled regression, where $RULE_{it}$ is an indicator variable that equals 1 if the observation is from the post-Rule 2711 period, and 0 otherwise; the t-statistics are computed using robust standard errors adjusted for clustering by company.

TABLE 7
Results from Regression of Monthly Coefficients for V^{RI}/P and V^{PEG}/P on Return from the S&P 500 Index

$$COEF\_VP_t = \beta_0 + \beta_1 RULE_t + \beta_2 SPRET_t + \beta_3 SPRET_t * RULE_t + \varepsilon_t$$

	COEF_VP = Monthly Regression Coefficient for			
	V^{RI}/P		V^{PEG}/P	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	−0.027	−1.96*	0.432	21.21***
RULE	0.244	14.45***	0.102	4.67***
SPRET	0.243	0.89	0.797	1.94*
*SPRET * RULE*	0.429	1.25	−0.241	−0.55
$\beta_2 + \beta_3$	0.672	3.21***	0.556	3.71***
Adjusted R^2	0.496		0.076	
Number of observations	144		144	

*, *** Significant at 10 percent and 1 percent levels, respectively (two-tailed).

$COEF\_VP_t$ equals the coefficient obtained from the regression of REC_{it} on $(V^{RI}/P)_{it}$ or $(V^{PEG}/P)_{it}$ for month t. REC_{it} denotes analysts' consensus stock recommendation for company i issued in month t (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). $(V^{RI}/P)_{it}$ is the intrinsic value of the stock for company i estimated in month t (as defined in Equation (1) of the text) divided by the closing stock price the day before the recommendation date in month t. $(V^{PEG}/P)_{it}$ is the consensus forecast of two-year-ahead earnings per share multiplied by the consensus forecast of the long-term growth rate in earnings, both for company i issued in month t, and divided by the closing stock price the day before the consensus recommendation date in month t. In the regressions, both $(V^{RI}/P)_{it}$ and $(V^{PEG}/P)_{it}$ are replaced by the monthly quintile ranking scaled to range between 0 and 1. $SPRET_t$ equals the return from the S&P 500 Index, cumulated from one day after the consensus recommendation date in month $t-1$ to the consensus recommendation date in month t. $RULE_t$ is an indicator variable that equals 1 if month t is in the post-Rule 2711 period (from May 2002 to December 2005), and 0 if month t is in the pre-Rule 2711 period (from January 1994 to April 2002). The sample consists of 144 month-observations.

t-statistics are computed using robust standard errors.

evidence to indicate that the changes in the relation between analysts' stock recommendations and V^{RI}/P and V^{PEG}/P after Rule 2711 are attributable to movements in the stock market.

Changes in Analyst Coverage

Our findings may be caused by changes in the companies that analysts cover. This is because when faced by pressure to issue fewer Buy recommendations and more Sell recommendations, some analysts might stop covering companies for which the stock recommendations are inflated. This type of change in analyst coverage may affect the relations of stock recommendations with V^{RI}/P and V^{PEG}/P, but it does not necessarily mean that the remaining analysts become more independent than before.

We address the issue of changes in analyst coverage in two ways. First, we replicate all the empirical analyses using a constant sample that consists of companies that are in the full sample for every month during the period 2001–2004.[19] The regression results (not shown) are similar to those presented in the paper. Second, we focus on the companies that have no change in analyst following after the implementation of Rule 2711. For each company, we use the number of analysts following the company in the last month prior to Rule 2711 (i.e., April 2002) as a benchmark and retain the observations from the same company with the same number of analysts. We replicate the regression analyses using this constant-analyst-following sample and the results (not shown) are similar to those presented in the paper.[20] Therefore, the results from these two additional tests suggest that our findings for the full sample are not driven by changes in sample companies or analysts' decisions to add or drop coverage of companies.

Computing V^{RI} Based on Alternative Assumptions

In our computation of V^{RI}, we treat the residual income in the terminal value year (RI_{t+5}) as persistent in perpetuity. This approach produces a higher intrinsic value compared with the approach that assumes mean-reversion for RI_{t+5}. To check the robustness of our results on the residual income model, we follow the procedure in Bradshaw (2004) and use industry-specific autoregressive parameters as an estimate of the persistence of RI_{t+5} in computing the terminal value. The autoregressive parameters are estimated based on the data for the ten years before issuance of the stock recommendation. The results (not tabulated) show that when V^{RI} is computed on the basis of mean-reversion in RI_{t+5}, there are downward shifts in the monthly coefficients on V^{RI}/P in Equation (2), but the mean of the monthly coefficients on V^{RI}/P is significantly higher for the post-Rule period and the coefficient on $(V^{RI}/P) * RULE$ in the pooled regression is significantly positive, both consistent with the results in Table 2.

In computing V^{RI}, we use discount rates that equal the industry cost of equity (Fama and French 1997) plus the risk-free rate at the time of the consensus stock recommendation. When we use a constant discount rate (ranging from 8 to 16 percent) to compute V^{RI} for all the sample observations, the results are similar to those reported in the paper, though the monthly regression coefficients on V^{RI} shift downward as the constant discount rate increases. Our conclusions, however, are unaffected.

The Relation between Changes in Stock Recommendations and Changes in V^{RI}/P and V^{PEG}/P

To mitigate potential problems of correlated omitted variables in the levels analyses, we run regressions of changes in stock recommendations on changes in V^{RI}/P and V^{PEG}/P (denoted by $\Delta(V^{RI}/P)$ and $\Delta(V^{PEG}/P)$, respectively). The results (not tabulated) show that (1) the mean of the monthly coefficients on $\Delta(V^{RI}/P)$ is significantly negative in the pre-Rule period but it is not significantly different from zero after the Rule, and the increase in the post-Rule period is statistically significant ($t = 1.80$), and (2) the mean of the monthly coefficients on $\Delta(V^{PEG}/P)$ equals 0.017 and 0.028 for the pre-Rule period and post-Rule period, respectively (both are significant), but the increase after Rule 2711 is not statistically significant (t = 1.25). In the pooled regression (results not tabulated), we find that (1) the coefficient on $\Delta(V^{RI}/P)$ is significantly negative and the coefficient on $\Delta(V^{RI}/P) * RULE$ is significantly positive, and (2) the coefficients on $\Delta(V^{PEG}/P)$ and $\Delta(V^{PEG}/P) * RULE$ are both significantly positive. Overall, the results from the changes analyses are consistent with those from the levels analyses.

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[19] This constant sample consists of 1,083 companies and represents 35 to 43 percent of the companies in the full sample in each month during the period 2001–2004. We choose this four-year period because a longer period substantially reduces the sample size. However, the results for the other years based on those 1,083 companies (subject to data availability) are similar to the results based on the full sample.

[20] In this constant-analyst-following sample, the number of observations is much smaller as month t is further away from April 2002. When we allow for a change in analyst following that equals 1 (provided that there are more than two analysts following the company), the sample size increases considerably, but the results are still similar.

In summary, we recognize that our findings of stronger relations between analysts' stock recommendations and intrinsic value estimates relative to the stock prices in the post-Rule 2711 period could be due to alternative factors unrelated to Rule 2711. We investigate several alternative factors and find no evidence that our main results are due to those alternative factors.

Conflicts of Interest Induced by Trading Incentives

In our primary analyses, we focus on the conflicts of interest that may be induced by investment banking opportunities. Since brokerage firms earn commission revenues from their clients' stock trading activities and issuance of favorable stock recommendations may promote stock trading, we investigate this source of a conflict of interest using either the trading volume or the turnover ratio of the company's stock as a proxy variable. The stock trading volume equals the total amount of the company's shares traded during the month before the consensus recommendation date, and the stock turnover ratio equals the total number of shares traded over the month before the consensus recommendation date divided by the number of shares outstanding. We run regressions of stock recommendations on the proxy for stock trading incentives as well as the valuation estimates and two proxies for conflicts of interest induced by future investment banking opportunities. The results (not tabulated) reveal positive relations of stock recommendations with stock trading volume and share turnover, and the relations are significantly weaker after Rule 2711 than before. The results for the other variables are unaffected. These results suggest that the influence of trading incentives on stock recommendations is attenuated after the implementation of Rule 2711.

Stock Recommendations and Future Stock Returns

Given the large decreases in the percentage of Buy/Strong Buy recommendations after Rule 2711, we also examine whether the investment value of analysts' stock recommendations (and of Strong Buy recommendations in particular) has improved since the regulatory change. Following Barber et al. (2003), we form five portfolios on the basis of recommendations, with each portfolio, 1 to 5, containing stocks with a consensus rating within the following five ranges, respectively: [1.0, 3.0), [3.0, 3.5), [3.5, 4.0), [4.0, 4.5), [4.5, 5.0]. The stocks in portfolio 1 are the least favored and the stocks in portfolio 5 are the most highly recommended. For each portfolio, we compute one-month-ahead abnormal returns that equal the value-weighted returns for the stocks in the portfolio from the day REC_t is issued to one day before REC_{t+1} is issued less the return from the value-weighted market portfolio over the same period.[21]

In Table 8, we show the means of the monthly abnormal returns to the *REC* portfolios and the percentage distributions of the portfolios before and after the implementation of Rule 2711. For the pre-Rule period (100 months), we also show the results for 1994–1999

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[21] Since we examine consensus stock recommendations, the date of *REC* in each month and the return accumulation period are the same for all the stocks. To be included in our analysis of stock returns, an observation must have daily stock return data available from CRSP for all of the trading days during the return accumulation period.

TABLE 8
Means of Monthly Abnormal Returns to Portfolios Formed on the Basis of Analysts' Stock Recommendations (p-values in Parentheses and Percentage of Sample in Bracket)

	Pre-Rule Period			
Portfolio and Range of *REC*	**1994 to Rule**	**1994 to 1999**	**2000 to Rule**	**Post-Rule Period**
Portfolio 1: [1.0, 3.0)	−0.52	−1.11	0.99	0.02
(Least favored)	(0.221)	(0.002)	(0.420)	(0.960)
	[1.7%]	[1.9%]	[0.9%]	[8.8%]
Portfolio 2: [3.0, 3.5)	0.29	−0.34	1.90	0.16
	(0.318)	(0.242)	(0.004)	(0.152)
	[17.4%]	[18.2%]	[15.0%]	[28.3%]
Portfolio 3: [3.5, 4.0)	0.17	−0.18	1.06	−0.07
	(0.382)	(0.236)	(0.061)	(0.459)
	[24.2%]	[23.6%]	[26.1%]	[29.0%]
Portfolio 4: [4.0, 4.5)	0.18	0.31	−0.17	−0.32
	(0.105)	(0.015)	(0.409)	(0.062)
	[34.3%]	[33.0%]	[38.4%]	[24.4%]
Portfolio 5: [4.5, 5.0]	−0.03	0.06	−0.28	0.69
(Most highly recommended)	(0.916)	(0.864)	(0.630)	(0.025)
	[22.4%]	[23.3%]	[19.6%]	[9.5%]
Portfolio 5 − Portfolio 1	0.49	1.07	−1.27	0.67
	(0.356)	(0.022)	(0.348)	(0.163)

REC denotes analysts' consensus stock recommendation (1 = Strong Sell; 2 = Sell; 3 = Hold; 4 = Buy; 5 = Strong Buy). Monthly abnormal return equals value-weighted return for the stocks in the *REC* portfolio, accumulated over the period from the recommendation date in the month to one day before the recommendation date in the next month, less the return from a value-weighted market portfolio of the NYSE/AMEX/NASDAQ stocks over the same period. The sample for the pre-Rule period consists of 307,969 recommendations issued between January 1, 1994 and May 9, 2002. The sample for the post-Rule period consists of 122,740 recommendations issued between May 10, 2002 and December 31, 2005. The returns reported in this table are means of the monthly abnormal returns over the indicated time period and are expressed as percentages.

p-values presented in parentheses are from two-tailed t-tests of the hypothesis that the mean (or the difference in the mean between two portfolios) equals 0. Percentages in brackets equal the number of observations in the portfolio divided by total observations for the same time period.

(72 months) and for 2000 to the month before Rule 2711 was implemented (28 months) separately because Barber et al. (2003) find that the relation between analysts' stock recommendations and future returns in 2000–2001 is different from that before 2000. Consistent with drops in the average stock recommendations after Rule 2711, the percentage of Sell recommendations (portfolio 1, *REC* < 3) increases from 1.7 during the pre-Rule period to 8.8 during the post-Rule period. On the other hand, the percentage of Strong Buy recommendations (portfolio 5, *REC* ≥ 4.5) decreases from 22.4 to 9.5.

During the pre-Rule period, portfolio 1 has the lowest average one-month abnormal returns but portfolio 5 does not have the highest, and the difference between the two extreme portfolios is not significant. When the pre-Rule period is divided into two subperiods, portfolio 1 has a significantly negative average one-month abnormal return during the 1994–1999 time period and this return is significantly lower than that for portfolio 5. For the subperiod from 2000 to the implementation date of Rule 2711, portfolio 1 has a positive average one-month abnormal return that is higher than the average for portfolio 5, though the difference is not statistically significant. In general, our results for the two subperiods before the implementation of Rule 2711 are consistent with the findings in Barber et al.

(2003). During the post-Rule period, portfolio 5 has the highest average one-month abnormal return and this portfolio is the only one that earns a significantly positive abnormal return.[22] Overall, the results in Table 8 provide some evidence that the investment value of analysts' Strong Buy recommendations is enhanced after the implementation of Rule 2711.

We also examine the one-month abnormal returns for quintiles 1 and 5 of V^{RI}/P and find that in both pre- and post-Rule periods, the mean abnormal return for quintile 5 of V^{RI}/P is significantly higher than that for quintile 1 of V^{RI}/P (results not tabulated). The predictability of V^{RI}/P for future abnormal returns is consistent with prior studies (Frankel and Lee 1998; Bradshaw 2004). These results complement our findings that the extent to which analysts incorporate V^{RI}/P into their stock recommendations has increased and that the portfolio of highly recommended stocks earns higher stock returns than do other portfolios in the post-Rule period.

VII. CONCLUSIONS

Analysts' tendencies to issue or maintain Buy recommendations for already overvalued stocks due to conflicts of interest prompted regulatory scrutiny and the subsequent implementation of NASD Rule 2711 in 2002. Rule 2711 aims to enhance independence of analysts' research, and a major provision in the Rule is to isolate analysts from pressures to generate investment banking business for their employers. In this study, we investigate changes in analysts' independence in making stock recommendations after the implementation of Rule 2711. We infer analysts' independence by the extent to which analysts incorporate the intrinsic value estimates relative to the stock prices (V/P) versus investment-banking opportunities into their stock recommendations.

Our empirical results show a significantly stronger relation between analysts' stock recommendations and V/P after Rule 2711 than before the Rule. In addition, we use the company's net external financing and the amount of underwriting business conducted by the analysts' employers as proxies for analysts' conflicts of interest and find that the relations of these two proxies with analysts' stock recommendations are significantly weaker after Rule 2711 than before the Rule. We further find that stock recommendations issued by analysts with greater potential conflicts of interest experience a greater increase in the relation with V/P after Rule 2711, which suggests that changes in analysts' behavior are greater for analysts who are more affected by the regulation. Additional tests show that the stronger relation between analysts' stock recommendations and V/P in the post-Rule period is not attributable to confounding factors and regulatory events, such as the implementation of Reg FD, analysts' substitution of investment signals, changes in market conditions, and changes in analysts' coverage decisions. Overall, our results suggest that the implementation of Rule 2711 has enhanced analysts' independence.

Our inference of analyst independence based on the relation between analysts' stock recommendations and V/P is subject to a few caveats. We estimate intrinsic values based on analysts' earnings forecasts to proxy for analysts' private valuations. This research design is based on the argument that analysts' earnings forecasts reflect their private information and, hence, that their intrinsic value estimates should be based on those forecasts. Prior studies based on analyst surveys and content analyses of analysts' research reports also find that analysts use earnings information when issuing stock recommendations (Bradshaw

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[22] We find that the portfolio of the stocks least favored by analysts observes positive but insignificant future returns, while anecdotal evidence suggests that analysts' Sell recommendations have earned large positive returns in recent years (*Wall Street Journal* 2005). The results we report are averages of the monthly value-weighted portfolio returns, which tend to be smaller than the average monthly returns of individual stocks.

2002; Asquith et al. 2005). However, our intrinsic value estimates may not reflect analysts' private valuations. Alternatively, the changes in the strength of the relation between analysts' stock recommendations and V/P may be caused by changes in the degree to which our value estimates reflect analysts' private valuations. This last alternative explanation could be valid if, when making equity valuations, analysts shift from one model to another depending on prevailing financial theories (Hunter and Coggin 1988) or market trends. However, such a shift would have to have occurred around the implementation of Rule 2711 in order to account for our results. While we cannot explore a complete list of alternative factors here, our additional tests and robustness checks do not show evidence that our main results are caused by the alternative factors that we do examine.

Note that prior research shows a large increase (decrease) in the percentage of analysts' Sell (Buy) recommendations and an increase in the profitability of their recommendations after Rule 2711 (Barber et al. 2006; Ertimur et al. 2007; Kadan et al. 2008). We demonstrate that analysts incorporate into their stock recommendations intrinsic values relative to the stock prices to a greater extent and investment banking opportunities to a lesser extent after Rule 2711 than before. Our findings of specific changes in how analysts generate stock recommendations contribute new evidence to the research on the effects of Rule 2711 on analysts' recommendations.

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Analyst Information Processing, Financial Regulation, and Academic Research

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ABSTRACT: Changes in regulations governing capital markets always provide a rich setting for archival researchers to examine how such changes affect the behavior of market participants. Barniv et al. (2009; hereafter, BHMT) and Chen and Chen (2009; hereafter CC) examine the impacts of recently enacted regulations aimed at curbing perceived abused by sell-side analysts. There were no less than six significant regulations issued between 2000 and 2003 that affected the activities of analysts. BHMT and CC emphasize different regulations, but both predict that analysts' recommendation will be less biased as a result of the collective regulatory changes. The evidence in both studies is strong and convincing that the association between analysts' earnings forecasts and stock recommendations has changed, consistent with analysts' personal conflicts of interest having less impact on their analyses. However, the attribution of what regulation, if any, effected this change is less clear. Collectively, the similarities and differences in the studies provide a nice setting to understand how different authors approach the same research question.

Keywords: *analysts; earnings forecasts; stock recommendations.*

I. INTRODUCTION

Academic studies of analysts' stock recommendations date back as far as back as Cowles (1932). For over half of a decade, such studies provide mixed evidence at best, with the majority of such studies concluding that recommendations have no investment value. For example, Cowles (1932) concludes that any investment returns to analysts' recommendation-based portfolios is due to chance rather than skill. Subsequently, Logue and Tuttle (1973) examine recommendations of six brokerage houses relative to a portfolio of randomly selected stocks, concluding that relying on brokerage firm recommendations yields returns commensurate with throwing darts at stock prices in newspapers. In contrast, it is only recently that data have become available to permit a large sample analysis of specifically identified recommendations (including release date). Using the then newly available First Call database, Womack (1996) provides a straightforward event-study

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analysis of the stock recommendations of sell-side equity analysts, and finds that upgrades (downgrades) are associated with three-day abnormal event returns of +3.0 percent (−4.7 percent). Additionally, he documents a post-event one-month drift of +2.4 percent for upgrades, and a longer six-month drift of −9.1 percent for downgrades. He concludes that the credible signal conveyed by an analyst's recommendation can be interpreted as the analyst saying, "I have analyzed the publicly available information, and the current stock price is not right" (Womack 1996, 164). Thus, this alternative view of analysts' recommendations provides a foundation for gaining a deeper understanding of how analysts' generate them.

The articles by Barniv et al. (2009; hereafter BHMT) and Chen and Chen (2009; hereafter CC) extend the literature aimed at providing a richer understanding of analysts' recommendations that has developed subsequent to Womack (1996). Both studies adopt a framework of analysis used by Bradshaw (2004), which attempts to shed light on how analysts generate stock recommendations using, among other things, their own forecasts of future earnings. The results in Bradshaw (2004) are startling (to some) and contrast with Womack (1996) because they provide a different perspective on the usefulness of analysts' recommendations at identifying misvaluation. Bradshaw's (2004) results suggest that an investor equipped with a simple spreadsheet and a few data points per firm (i.e., book value, the analysts' earnings forecasts, a cost of capital estimate, and current stock price) can easily come up with valuations (i.e., a "V/P" ratio, as in Frankel and Lee [1998]) that dictate an investment strategy that outperforms one based on analysts' recommendations. The explanation for the wedge between the affirmative results in Womack (1996) and dissenting results in Bradshaw (2004) is that the former focuses on a short-window changes setting with active portfolio buying and selling, whereas Bradshaw (2004) focuses on a long-window levels setting with a buy-and-hold investment approach. To my knowledge, a full reconciliation of the differences between short-window and long-term changes versus levels approaches is not present in the literature.

BHMT and CC couple an interest in analysts' recommendations with a long-existing undercurrent of suspicion among academics and regulators that analysts are subject to conflicts of interest that at best reduce the usefulness of their outputs and at worst makes them dangerous to unsuspecting investors.[1] Both studies examine whether recent regulation (discussed further below) aimed at limiting analysts' conflicts of interest has resulted in a normalization of the counterintuitive findings in Bradshaw (2004), such that analysts' recommendations are better supported by fundamentals than prior to such regulation. BHMT and CC replicate the original results in Bradshaw (2004) for the pre-regulation period, and then show that after the introduction of regulation, the results are more in line with the spirit of Womack (1996). Thus, both sets of authors conclude that analysts' recommendations now provide a more useful signal regarding misevaluation than in years prior to recent regulatory actions, consistent with the conclusion that recent regulation of analyst activities has had a positive impact on the investment usefulness of analysts' recommendations.

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[1] This skepticism of the value of analysts is pervasive beyond just academics and regulators. For example, one of my favorite fiction authors is Tom Robbins, who wrote in *Half Asleep in Frog Pajamas*, that it is "common knowledge that brokers push stocks and other instruments based on what the brokerage firm wants to peddle on a given day or week. It's also common knowledge that unsuspecting customers are sold specific securities ... that result in the highest commission for the broker. It's a fact of life, sad but true, that most brokers have their own interests at the forefront of every buy and sell recommendation that they proffer. That just goes with the territory, I guess" (Robbins 1994, 88).

The studies are remarkably similar in motivation, research design, and results. What would typically happen in circumstances where two "competing" studies are simultaneously under review is that one would receive short shrift and be forced to change very substantially or die a silent death.[2] However, it is enlightening to witness, side-by-side, two studies that were independently prepared. I aim to do several things in this commentary. First, I discuss the related literature to which these studies contribute in general. Second, I address the particular regulatory-driven framework incorporated in the studies and provide some tempering thoughts on the ability to attribute results to regulatory changes. Then, I compare and contrast BHMT and CC from a research design perspective, and include some perspective on the preparation of the Bradshaw (2004) study that both studies use as motivation. Finally, I offer some preliminary thoughts on how the literature on analysts might best proceed to both extend our current understanding of the role and influence of analysts and, perhaps as important, maintain the interest of other researchers who are interested in studies of capital markets in general.

II. CONTRIBUTION TO THE LITERATURE ON SELL-SIDE ANALYSTS

Sell-side equity analysts motivate a substantial number of published academic research articles. In more recent studies (e.g., past decade), the majority of these articles appear in what are typically considered accounting journals, rather than those considered finance or economics journals, where such research could just as logically be concentrated. After all, sell-side analysts typically work at investment banks that serve as conduits for all sorts of corporate finance transactions. Accounting researchers are interested in analysts because analysts are assumed to be one of the primary users of financial statement information. Additionally, the fact that analysts provide forecasts of accounting earnings enables analysts to serve as proxies for expectations of investors in general, which benefits all kinds of research questions regarding whether and how various financial information gets assimilated in capital markets. Thus, although finance and economics researchers display an interest in understanding the activities of sell-side analysts, accountants seem to have cornered the market on studies of analysts, for better or worse.

Despite our having cornered the market on analyst research, my observation is that many accounting academics have grown impatient or tired of academic research on analysts. Several anecdotes should make this clear. First, I presented a study at one of our premier universities in 2005, and was told in a tone of exasperation, "Every paper this semester has been on analysts!" Second, at a conference held at another fine business school, one presenter began a talk with a slide enumerating why he liked that conference, and the first bullet point was, "No analyst papers." Third, at a meeting of the Financial Accounting Reporting Section in 2006, one of the more level-headed senior faculty members in our profession characterized the literature on analysts as "long in the tooth." Finally, prior to delivering an invited commentary on analyst research, I was told by one of the preeminent contributors to this literature that, "You have been assigned the topic held in the most disdain by researchers." Given this climate, an important pair of questions is, "What is wrong with this literature?" and "Can anything be done to save it?"[3]

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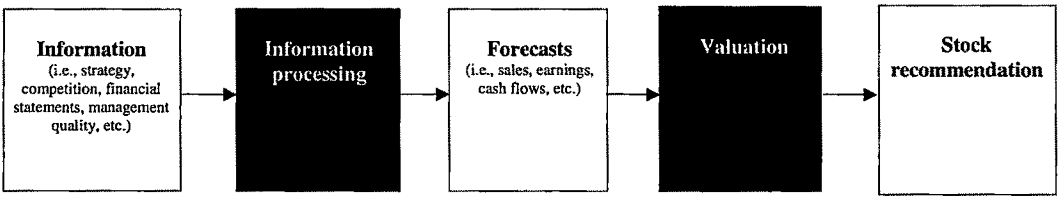
[2] Actually, the outcome would not be as bad as a "silent death," as the authors of the slighted study would likely publish their study in a similar or "watered down" version elsewhere. However, the other study would likely end up being the one more frequently cited in subsequent research. An analogy is the music industry, in which the arrival of contemporaneous performers who share similar styles and merit inevitably leads to one being well-known and the other being relegated to secondary status (e.g., Elvis Presley versus Cliff Richard in the 1950s, Bob Dylan versus Donovan in the 1960s, Bee Gees versus Leo Sayer in the 1970s, U2 versus Big Country in the 1980s, Sheryl Crow versus Shawn Colvin in the 1990s, etc.).

It seems to me that one of the primary reasons researchers have become exasperated with analyst research is that too many studies seek to investigate whether a particular piece of information (e.g., research and development expense, stock option expense, segment disclosures, accruals) is fully understood by investors, which is empirically tested by regressing analyst forecast errors on the piece of information (and perhaps other control variables). Consequently, such studies collectively imply that analysts are primarily focused on getting earnings forecasts correct. However, Schipper (1991) pointed out early on in this literature that one of analysts' ultimate roles is to provide an assessment of the investment worthiness of a particular stock, and earnings forecasts are an input into this decision process. Not surprisingly, this is something that both BHMT and CC highlight in their motivation. Accordingly, the shift in focus of these two studies away from earnings forecasts toward other outputs is a welcome change in this research. Moreover, rather than a simple change in the left-hand side variable from earnings forecasts to recommendations, these two studies attempt to incorporate analysts' other observable outputs into the analysis of recommendations.

This shift by BHMT and CC is intentional, because the research question centers on analysts' actual decision processes. Figure 1 provides a simple schematic to capture the focus in these studies. The process begins with the availability of public information such as firm strategies, the competitive landscape, financial data, and other nonfinancial factors like quality of firm management. Armed with this information, analysts invoke their skills to analyze various data and derive quantified expectations of future earnings. Then, the analysts further invoke their skills to process these earnings forecasts into firm valuations, which, when compared to the current trading price, result in a justifiable stock recommendation that is released to investors. Of the five steps in this simple schematic, the most interesting ones happen to be the "black boxes" in Figure 1—the analysis that leads to earnings forecasts and the analysis that leads to the stock recommendation. The information processing that leads to the earnings forecasts is implicitly of interest in the studies by BHMT and CC; however, the research focus is on the information processing that occurs in the fourth box that generates stock recommendations.

FIGURE 1
Simple Schematic of Analyst Information Processing


Information
(i.e., strategy, competition, financial statements, management quality, etc.)
Information processing
Forecasts
(i.e., sales, earnings, cash flows, etc.)
Valuation
Stock recommendation

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[3] In another article, I discuss a number of aspects of the trajectory of the accounting literature on analysts (Bradshaw 2008). The discussion that follows builds upon one recommendation in that article, which is relevant to the assessment of the contributions of BHMT and CC, which is that both studies offer a more "holistic" approach to understanding analysts' activities rather than a singular focus on, say, earnings forecast accuracy.

This is the black box that we as researchers are keenly interested in, but which is unfortunately not easily penetrated by empirical archival research methods. As in Bradshaw (2004), both BHMT and CC presume that correlating recommendations with researcher-computed valuations (relative to price) will give insight into how analysts use their earnings in determining valuations that presumably generate observed stock recommendations. Archival research methods *may* support conclusions regarding how analysts process information into their forecasts and recommendations, but it is not unequivocal. Larcker and Lessig (1983) make this point nicely in an experimental study that asks subjects to make a stock recommendation based on certain financial information. The authors then examine two methodologies for explaining the subjects' recommendations: linear modeling (i.e., regression analysis) and retroactive process tracing techniques (i.e., interviews with subjects). The results of their analyses are intriguing and relevant to the literature to which BHMT and CC contribute. Although both methods explained the resulting recommendations, the explanatory variables that were important were not the same across the two approaches. Larcker and Lessig (1983) conclude that if one is interested in predicting an outcome like a recommendation, then linear regression approaches work well. However, if one is interested in how decisions are made (i.e., the "black boxes" in Figure 1), then alternative methods seem necessary.

The good news is that the financial accounting literature that utilizes experimental methods is growing. Stemming from the 1990s, an increasing number of accounting researchers have been trained in behavioral (and experimental) research, and such studies are predominantly aimed at understanding features of markets that affect participant behavior and information processing (Bloomfield et al. 2002). There are limitations to experimental research designs as well, so this methodology is not a panacea for the limitations of empirical archival work. Instead, these approaches seem to be complementary, providing a compelling argument for those who typically do archival work to consume the research of those that perform experiments. A recent article by Koonce and Mercer (2005) provides a framework that highlights many of the theories that might be most useful to empirical archival research studies.

III. A REGULATORY QUESTION ... OR NOT

Both BHMT and CC ask what effect regulation has on analysts. CC partition their primary analyses around NASD Rule 2711 and the amended NYSE Rule 472, which were both approved on May 10, 2002 and implemented in phases between July and November 2002. These two rules prohibited a number of activities that were believed to have caused analysts to provide biased research in the years leading up to the market peak during the summer of 2000. BHMT, on the other hand, do not partition based on any single regulation, but center their analysis on "Regulation Fair Disclosure (Reg FD) and other recent regulatory reforms (e.g., including NASD Rule 2711 and NYSE Rule 472, as well as the Global Research Analysts Settlement)." Reg FD was issued in October 2000, and the $1.4 billion Global Analysts Research Settlement was announced in finalized in April 2003. In addition, the Sarbanes-Oxley Act was issued in 2002 and may have affected the information environment for analysts. Finally, the SEC also implemented Regulation Analyst Certification, which required disclosures of potential conflicts surrounding compensation and became effective during the same month the Global Research Analyst Settlement was reached. Thus, there are no less than *six* significant regulations that affected analysts' activities, the earliest of which was Reg FD in October 2000, and the latest of which was the $1.4 billion settlement in April 2003.[4]

The primary hypothesis in both studies is based on the premise that analysts were misbehaving and that regulation was issued to curb this bad behavior, which leads to a predicted change in the analysts' behavior. Here, the change in behavior being predicted is that analysts were issuing stock recommendations that were unsupported by "legitimate" valuations of the analysts' own earnings forecasts. One must first ask whether the specific regulation being examined could logically explain the predicted change in behavior. Both NASD Rule 2711 and NYSE Rule 472 seem well suited for this purpose, as the stated goal of both was to curb perceived abuses by analysts in terms of touting stocks that they privately believed to be poor investments. The Global Analyst Research Settlement was similar in spirit, but reflected punishment for perceived wrongdoings that NASD Rule 2711 and NYSE Rule 472 were meant to curb. Reg FD was meant to prevent differential information access to firm insiders, and the common view is that it is meant to prevent analysts from getting information from management that is not publicly available to investors at large. In this sense, it is not obvious why this regulation would generate a prediction that analysts' recommendations would be more closely aligned with valuations using various models. BHMT cite Herrmann et al. (2008), who document that analysts' previous incentives to provide optimistically biased forecasts to curry favor with management was reduced after Reg FD, which they argue increases the usefulness of earnings forecasts as inputs to valuation models (e.g., the first black box in Figure 1). However, it is difficult to believe that the magnitude of any change in such forecast bias could have any appreciable effect on valuations. Nevertheless, BHMT also provide a parallel set of analyses with an alternate sample period partition that is meant to capture the other regulations as well.

In summary, all of these regulations, with the possible exception of Reg FD, seem to be reasonable bases for predicting changes in analysts' behavior. As long as the effects of these regulations can be isolated from other events that might also have affected analysts' behavior, it would be reasonable to attribute observed changes in analyst behavior to the regulation. However, this separation is not possible in these studies, or for that matter, in others that examine the impact of regulation. The problem seems particularly acute in the authors' setting, due to the large number of contemporaneous regulations that affected analysts. Clearly, these regulations were not drafted in a vacuum. Rather, they followed a number of other events, most notably the peak in the stock market in June 2000 and the associated burst of the "dot.com bubble." Thus, any change in associations between analysts' recommendations and valuation estimates might be attributable to other, primitive events like the stock price crash or white collar criminal prosecutions that followed. If so, it is problematic to conclude that the regulations per se effected the change in analysts' behavior.

Both sets of authors are aware of this problem. BHMT are more forthcoming in referring to the collective regulations, whereas CC examine NASD 2711 and NYSE 472 specifically, but provide sensitivity tests for other regulation. Nevertheless, the results in both studies should be viewed as suggestive with respect to the benefits of regulation, because the documented changes may be due to other factors altogether. In empirical research, this is the best one can do. The alternative is to not do such research, which is clearly not helpful. The burden then lies with future researchers to not neglect the appropriate caveats included in studies like these.[5]

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[4] CC do not ignore Reg FD or the Global Analyst Research Settlement, but address these alternative timelines in sensitivity analyses.

IV. COMPARISON OF SPECIFIC RESEARCH DESIGN CHOICES IN BHMT AND CC

Similarities

I had planned on preparing a table that compared the various motivation and research design differences between BHMT and CC, with the objective of highlighting differences in the studies. However, in doing so it became clear that the key aspects of the research design choices are almost perfectly correlated. Thus, it is easier to provide a bullet-point list of key aspects of the studies that are essentially identical, followed by a brief highlight of several differences. Both studies reflect the following:

Research Design Choices	**Description**
Motivation	Recent regulations have the objective of altering the behavior of sell-side analysts. Previous research provides evidence consistent with analysts' inefficient use or intentional misuse of their own forecasts in generating stock recommendations.
Research question	Did the regulations make analysts' recommendations better, in the sense that they capture fundamental valuations that incorporate the analysts' forecasts?
Empirical proxies for changes in analysts' behavior	corr (Recommendations, V/P), where V = valuation model estimate based on analysts' own earnings forecasts (i.e., α_1 and α_3 regression coefficients, see below).
Assumptions	Estimates of V capture analysts' private valuations.
Valuation models	V^{RI} and V^{PEG}.
V^{RI} assumptions	V^{RI} is based on a five-year forecast horizon with industry-based cost of capital estimates and a perpetuity of terminal year residual income.
V^{PEG} assumptions	$V^{PEG} = EPS_2 * LTG$.
Regression specification	Recommendations $= \alpha_0 + \alpha_1 V/P + \alpha_2 REGULATION + \alpha_3 V/P \times REGULATION + \varepsilon$, where *REGULATION* is an indicator variable for the post-regulation period.
Sample period	1994–2005.

Following Bradshaw (2004), the authors of both studies examine the following valuation models: (1) a residual income valuation model and (2) a PEG valuation model. A restriction of this analysis is that it presupposes that these are the valuation models that analysts use, the model parameters are descriptive (i.e., forecast horizons, cost of capital, terminal value assumptions), and more importantly, there is an implicit assumption (in the cross-sectional research design) that valuation model use is constant across firms.

An obvious question is why the studies use this set of valuation models. The authors' response might be that they follow the approach in Bradshaw (2004). As the author of that study, I can respond somewhat to this question with a brief "my diary" discussion of the preparation of that study, because the initial referee report from *The Accounting Review* indeed questioned why the residual income and PEG valuation models were the chosen models.[6] When I settled on the research question to ask whether analysts' recommendations seem to be generated based on straightforward valuations of their own earnings forecasts, I anticipated that I knew the answer (i.e., of course they do). Consequently, I expected to provide a careful study that used the then re-popularization of the residual income valuation methodology (Ohlson 1995) to show that analysts' recommendations could be explained by such valuations. Despite numerous combinations of valuation model assumptions (i.e., forecast horizon, cost of capital estimates, terminal value assumptions), the results all indicated a negative association between V/P estimates and analysts' recommendations.

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[5] See Bamber et al. (2000) for an excellent discussion of how we, as researchers, often tend to subsequently reference papers in sound bytes that ignore important research design choices or caveats that authors highlight in their studies.

To gain further insight, I interviewed numerous analysts and performed a content analysis of 101 randomly selected research reports. This exercise revealed a marked use of a PEG heuristic, which had almost no presence in academic literature at the time. If analyses were re-performed with PEG valuations substituted for the residual income valuations, then the sign of the association with recommendations turned significantly positive (as predicted) and the explanatory power increased. Thus, the justification for using these models as proxies for analysts' private valuations was that they bracketed a range of possible models: the residual income model is a simple algebraic form of the widely accepted dividend discount model (with a clean surplus assumption), whereas the PEG model is a heuristic that is difficult to link directly to the dividend discount model.[7] To respond to the referee concern that PEG was not well-motivated in the literature, I wrote up the results of the content analysis and submitted it to *Accounting Horizons* (Bradshaw 2002).[8] Thus, although one might still question why alternative models (e.g., price-to-sales, price-to-EBITDA, discounted free cash flow versions) are not considered in BHMT or CC, the research question they are asking justifies the restriction to earnings-based models. Further, because both studies are primarily interested in an across-time analysis, the benchmark model is less relevant due to the nature of the change in associations that they document.

Differences

There are three noteworthy differences, which are as follows:

Research Design Choices	**Description**
Sample size	Both studies use I/B/E/S data, but CC use 433,152 firm-months and BHMT use 187,889. The difference is that BHMT retained only firm-months during which the consensus changed, which should be a more powerful sample in light of the changes results in Womack (1996).
Valuation models	In addition to V^{RI} and V^{PEG}, BHMT use a second V^{RI} model and another heuristic representation of a valuation model, the analysts' long-term growth projection (*LTG*).
Sample period split dates	CC focus on NASD 2711 and NYSE 472, and split their sample into periods before and after May 10, 2002 (the date of SEC approval). BHMT, who focus on "Reg FD and other recent regulatory reforms," partition their sample period into 1993–1999 and 2000–2005 for the pre- and post-Reg FD periods, and also a 2000–2002 and 2003–2005 partition for the pre-other and post-other regulation periods.

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[6] The initial submission to *The Accounting Review* was actually written in this fashion, and generated a blunt critique from the editor that "this paper suffers from the 'my diary' problem." It was a well-placed criticism.

[7] Subsequent to me making this argument, Easton (2002) shows that PEG-valuations can be tied to the dividend discount model and are able to rank-order stocks in terms of the market's expected return.

[8] This substantially increased the efforts and time to publish Bradshaw (2004), but the publication of Bradshaw (2002) seemed to be the only way to solve a circular problem of justifying the choice of valuation models.

As it turns out, these differences do not significantly change the key takeaways. However, these differences do have an effect on the interpretation of the sign of the correlation between recommendations and valuation estimates (but not the change in correlations). Although the sample size may contribute somewhat to variation in estimates of the sign of the correlation (i.e., regression coefficient on V/P), the most substantive effects are driven by the valuation model assumptions (i.e., terminal value) and the sample period partitions. The latter follow from the different stated regulatory focus in each of the studies.

After the regulations, the reported signs of the estimated correlation between recommendation and residual income valuations are positive in CC but negative, zero, or positive in BHMT, depending on the valuation model and sample period cutoff. The results in Bradshaw (2004) show a negative correlation, which is opposite to what one might expect. Both of the authors primarily emphasize the *directional change* in the sign of the correlation between recommendations and fundamental valuations, predicting that it will become "more positive," such that the actual sign of the correlation is emphasized less. For V^{RI1}, BHMT report a coefficient of −0.304 in the pre-regulation period, and −0.117 (= −0.304 + 0.187) in the post-regulation period. On the other hand, CC report a coefficient on V^{RI} of −0.024 in the pre-regulation period, and +0.219 in the post-regulation period.

It appears that both the terminal value assumption and the different sample period partition dates matter here. First, when BHMT use a terminal value assumption that presumes a perpetuity of residual income (as in CC), the results for the post-regulation period become positive (+0.039 = −0.186 + 0.225). Second, because BHMT examine Reg FD and other regulations, they also report results in their Table 3 that use the 2000–2002 and 2003–2005 cutoffs to better isolate the implementation of NASD Rule 2711 and NYSE Rule 472. These results are closer in spirit to those of CC, with coefficients on V^{RI} of −0.206 in the pre-regulation period, and 0.000 (= −0.206 + 0.206) in the post-regulation period. The combined effects appear in the second column of Table 3, where BHMT use a perpetuity terminal value and a partition around other regulations, and they find a change in the coefficients from −0.093 to +0.200 (= −0.093 + 0.293).

The interesting takeaway from this discussion of the tabulated results is that research choices can have a significant impact on conclusions. BHMT and CC both hypothesize a directional change in the regression coefficient on V/P, not the level of the coefficient, and both studies provide strong evidence that the directional change is significantly positive and robust to numerous design choices. However, were the focus to have been on the level of the coefficients, as in Bradshaw (2004), the results would be less robust.

V. CONCLUSION AND NEXT STEPS

Both Barniv et al. (2009; hereafter BHMT) and Chen and Chen (2009; hereafter CC) provide interesting evidence on a distinct change in characteristics of recommendations before and after periods during which several regulations were issued that provided tighter oversight of analysts' activities. The overall result common to both studies is that the perverse association between valuation estimates and analysts' recommendations initially documented in Bradshaw (2004) has dissipated in recent years. Due to the various sensitivity tests provided in both studies, these results are very convincing. The million-dollar question is whether the results can be attributed to the new regulations. The fact that both articles emphasize different regulations tacitly makes the point that it is difficult to make such an attribution. The number of different regulations and their overlapping introduction and implementation creates an inability to attribute the observed change to any single piece of regulation. CC seem to overemphasize NASD Rule 2711 and NYSE Rule 472 and

BHMT tend to overemphasize Reg FD while simultaneously acknowledging "other" regulations as well. Both studies acknowledge, in different ways, that there were numerous regulations and it is hard to pin down any single one. As noted above, it is very important that future research not selectively reference either piece of regulation as a causal force that changed analysts' behavior, which would ignore the well-balanced discussions in both BHMT and CC (e.g., Bamber et al. 2000).

The interest conveyed by the authors of these studies is in the information processing and behavior of analysts. Understanding how users of financial statements respond to and process accounting information is an exceptionally pervasive goal within the accounting literature, stretching beyond studies restricted to a focus on sell-side analysts. Although there are limitations to how much linear regression models can speak to the actual processing of accounting information and associated decisions, the complementary evidence available in studies published by colleagues in the experimental accounting sub-field ought to be collectively included in any conclusions that we wish to draw from empirical results. More importantly, there are a large number of behavioral theories available that might be productively included in future research (e.g., studies interested in how analysts process accounting data and transmit the results of their analysis to markets). Together, BHMT and CC contribute to this literature by examining a change in behavior surrounding a flurry of events that any such theories would predict would result in the changes documented by the authors.

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The Effect of Auditor Quality on Financing Decisions

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ABSTRACT: We present a model and provide empirical evidence showing that auditor quality affects the financing decisions of companies, and that higher audit quality reduces the impact of market conditions on client financial decisions and capital structure. Consistent with our analytical predictions, we find that companies audited by Big 6 firms are more likely to issue equity as opposed to debt than are those audited by small audit firms. We also find that companies audited by Big 6 auditors are able to make larger equity issues than are those audited by small auditors, but the difference narrows when market conditions improve. Additional results show that the debt ratios of companies decrease less in response to favorable market conditions when auditor quality is high, at least over the medium term.

Keywords: *auditor quality; financial decisions; adverse selection; capital structure.*

Data Availability: *All analyses are based on publicly available data.*

I. INTRODUCTION

The theoretical literature has long recognized that the asymmetry of available information between companies and outside investors can affect companies' financing choices. For example, Myers and Majluf (1984) explain how adverse selection can

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lead companies to refuse to issue equity and forgo profitable projects. A company's financial statements play a critical role in reducing this asymmetry, and their integrity is essential to well-functioning capital markets. In turn, auditors play a key role in assuring the integrity of information. Yet, all auditors may not offer the same level of service. While the services of larger auditors (the Big 6 firms, in particular) are more expensive (as shown, for example, by Ireland and Lennox [2002]), these firms are usually thought to offer a higher level of audit quality.[1] For example, Willenborg (1999) states that it is widely perceived that larger, more prestigious firms have greater incentives not to perform a low-quality audit at a high-quality price. This higher quality should reduce the information asymmetry between informed managers and uninformed suppliers of capital and thus affect a company's financing decisions.

We investigate how differences in auditor quality can affect the timing and the choice of security issuance. We develop a model that is based on the idea that the quality of the auditor can reduce the information asymmetry about future earnings of the company. Our framework is based on time-varying adverse selection. This adverse selection implies that companies audited by a poor-quality auditor will rely more on debt as opposed to equity when compared to companies that are audited by higher quality auditors. Moreover, we show that the average size of equity issuance by companies audited by low-quality auditors will also be smaller than that of companies audited by high-quality auditors. The model also suggests that, irrespective of its auditor's quality, a company relies more on equity issuance when the market is optimistic about the its prospects. This result stems from the fact that the degree of adverse selection is lower in good economic periods. However, because the average degree of adverse selection is higher for companies that are audited by low-quality auditors, the equity issuance decisions of such companies will be more sensitive to market conditions. Specifically, the difference in the size of equity issues between companies audited by high- and low-quality auditors will narrow when the market is optimistic about the prospects of companies.

Our empirical results are consistent with these predictions. First, we find that companies audited by Big 6 firms are more likely to issue equity as opposed to debt than are those audited by non-Big 6 firms. Second, the size of equity offerings is smaller for companies that are not audited by Big 6 firms. While the size of equity offerings increases for all companies when stock returns in the recent past are higher, the effect is stronger for those that do not hire Big 6 auditors. These results are both economically and statistically significant. We explore the consequences of our main findings on the company's capital structure For example, we find that the debt ratios of companies increase less over a five-year window when stock returns improve. However, the effect of stock returns is again stronger for companies that do not hire Big 6 auditors. We also find that companies audited by Big 6 firms have less debt in their capital structures.

One concern is that the auditor quality may be endogenous and company characteristics, company size in particular, may drive observed effect of the auditor size. We perform numerous empirical tests to establish the robustness of our empirical results in relation to these potential problems. First, we address the possibility that company size may drive our results. To do so, we replicate our results in a sample matched on company size and two-digit SIC industry codes. Our results hold. Second, we replicate our Ordinary Least-Squares (OLS) tests using different econometric specifications specifically designed to control for the endogeneity in the auditor choice. A traditional approach is to use the Two-Stage Least-Squares (2SLS) procedure. However, our endogenous variable is binary, which precludes the standard approach. Instead, we employ two alternative strategies. In the first strategy, we predict the likelihood of the company to select a Big 6 auditor in the first stage and then we use this predicted value in the second stage. We bootstrap the system 500 times to obtain consistent standard errors. In the second strategy, we follow Wooldridge (2002) and use the predicted value of the auditor quality as an instrument in the instrumental regressions. All results hold. Third, we consider a sample of firms for which auditors have long tenure. Given that the size of the auditor has been established for many years for these firms, the possibility that auditor quality is endogenous with respect to the current financial decisions of the firm is greatly reduced. All our results hold. Fourth, we consider different panel specifications and a sample of firms that switch the size of their auditors. Results from these specifications indicate that the effect of external conditions on issuances or the company's capital structure is mitigated for companies audited by Big 6 auditors. The fact that our results are robust to company fixed effects indicates that they are not driven by time-invariant omitted variables.

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[1] We follow the convention of calling the main firms the Big 6. The actual number of main firms has varied over time from eight in 1980s to four currently due to several mergers and the dissolution of Arthur Andersen.

While the theoretical literature has suggested that Big 6 auditors serve a valuable function, there is a paucity of empirical evidence on the importance of auditor quality to companies' financing decisions. Our empirical results show that auditor quality is relevant for companies' financing decisions; furthermore, the tests exploit the fact that the extent of adverse selection can vary with the market conditions that a company is facing and therefore the importance of auditor quality is also likely to vary with these market conditions. Our finding that companies with Big 6 auditors rely more on equity financing relative to debt financing and have lower debt ratios compared with companies not audited by a Big 6 firm relate to recent findings that auditor quality affects both the cost of equity in Initial Public Offerings (e.g., Willenborg 1999; Copley and Douhett 2002) and the cost of debt (e.g., Mansi et al. 2004; Pittman and Fortin 2004). Looking at debt-equity choices essentially allows us to consider the cost of equity relative to the cost of debt instead of trying to evaluate the absolute cost of equity. Therefore, showing that auditor quality has a first-order effect on the debt-equity choice extends the prior results outside the IPO setting. Finally, our results are generally stronger for smaller companies. For these companies, auditors are likely to play a key role since there are no additional outside informational intermediaries between managers and investors. This stands in contrast to Chang et al. (2006), who examined the effect of variations in analyst coverage on financial decisions. In their study, most of the variation occurs among companies that are large enough to attract some coverage. In our study, most of the variation occurs among small companies.

Section II describes our hypotheses and methodology. The sample and variables are discussed in Section III. Section IV discusses our empirical results. Section V provides an investigation of the causes and the consequence of these results. Section VI reports different robustness checks. Section VII concludes.

II. HYPOTHESES DEVELOPMENT AND EMPIRICAL SPECIFICATIONS

To motivate our main testable hypotheses, we outline a model based on the idea that current earnings serve as estimates of future earnings and that the quality of the audit of current earnings affects information asymmetry about future earnings. The basic intuition behind the model is that companies sell equity instead of debt when they face low adverse-selection costs. Both good economic prospects and a high-quality auditor can mitigate this adverse selection and facilitate the issuance of equity. The model is similar in spirit to the time-varying adverse-selection model of Choe et al. (1993).

Model Outline

Cash Flows, Projects, and Information Structure

We consider a one-period model with two dates, $t = 0$ and $t = 1$. Earnings are reported at $t = 0$. Let X_0 denote the reported earnings. We assume that there are two possible states of nature at $t = 0$, denoted by *Low* (L) and *High* (H). These states of nature determine true earnings (X) in a non-shock state of the world at $t = 1$. Namely, if there is no shock to the economy at $t = 1$, then the true earnings will be X_L and X_H in the *Low* and *High* states, respectively ($X_L < X_H$). The *ex ante* probability that $X = X_H$ is p, where $0 < p < 1$. The state of nature is privately known to the company's managers at $t = 0$. We assume that reported earnings X_0 can be either X_L or X_H. With exogenous probability δ, the non-shock state of the world prevails at $t = 1$ and earnings are X. With the complementary probability $1 - \delta$, a shock occurs and earnings are 0.[2]

We assume that there are two possible projects at $t = 0$. The projects require an initial investment of $I = I_1$ or $I = I_2$, where $I_1 < I_2$, at $t = 0$. Only one of the projects is available at $t = 0$ to a particular company, and availability is determined randomly with an exogenous probability q. The type of project that is available is public information. The projects have identical earnings. The earnings from the project are realized at $t = 1$. In what follows, the earnings from the project are distinguished from the earnings from *existing assets*, which can be either X or 0 at $t = 1$, as discussed above. We refer to the earnings from existing assets as "other earnings." For the new project, there are also two possible outcomes (shock and non-shock) at $t = 1$. The project earnings are $I + V$ if the non-shock state of the world prevails at $t = 1$ (and the other earnings are X), and 0 if the shock state occurs (the other earnings are then also 0). The net present value (NPV) of the additional project is therefore $\delta(V + I) - I$. The assumption that the NPV, $\delta(V + I) - I$, of the project is decreasing in project scale I captures the notion of diminishing returns to scale.

Earnings Manipulation, Auditor Quality, and Managerial Objectives

True earnings X in the non-shock state of the world need not be the same as reported earnings X_0. Companies can boost the market's perception of actual earnings through accounts manipulation. By assumption, this is only possible if the true earnings are X_L. If the company is successful in such manipulation, then its reported earnings are X_H. A good-quality auditor will always detect any misreporting, so its client's announced earnings are the same as its true earnings. A poor-quality auditor, however, detects manipulation with only a probability of 0.5.[3] Auditor quality is assumed to be public information. We assume that managers act in the interest of existing shareholders in deciding the type of security to issue. The company finances the new investment either through the issuance of debt or equity. We assume that the cost of financial distress (if the company issues debt and is in default) is $c > 0$.[4]

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[2] Put differently, there are four possible states of the world at $t = 1$: *High* and *Non-Shock* (*HN*), *Low* and *Non-Shock* (*LN*), *High* and *Shock* (*HS*), and *Low* and *Shock* (*LS*). The manager knows whether the state is *High* or *Low*, but does not know whether it is *Shock* or *Non-Shock*. In addition, $X_{HN} = X_H$, $X_{LN} = X_L$, and $X_{HS} = X_{LS} = 0$.

[3] The assumptions that the good-quality auditor detects manipulation with a probability of 1 and the poor-quality auditor with a probability of 0.5 are without any loss of generality, and are made for simplicity of exposition. All of our results hold when good- (poor-) quality auditors detect misreporting with a probability of m (n), as long as $m > n$, but these extra parameters are not needed.

Debt-Equity Choices

By assumption, when auditor quality is good, earnings manipulation is not successful, and the reported earnings X_0 at $t = 0$ must equal X. Hence, by construction, there is no adverse selection associated with equity issuance. However, debt issuance is always associated with the expected cost of financial distress of $(1 - \delta)c$. Therefore, managers acting in the interest of existing shareholders will issue equity when auditor quality is good, irrespective of the scale of the project (i.e., whether I_1 or I_2).

Now suppose that auditor quality is poor. If reported earnings at $t = 0$ are $X_0 = X_L$, then this must be the true state of earnings. Consequently, again, there is no adverse selection with respect to equity issuance, and the company will issue equity to finance its projects.

If, however, auditor quality is poor but reported earnings are $X_0 = X_H$, then the market does not know whether the true earnings are X_H, or that the true earnings are X_L but earnings manipulation is successful. The market, therefore, takes into account that a poor-quality auditor detects manipulation with probability of 0.5 and that the *ex ante* probability of true earnings being high is p to form a (Bayesian) estimate of the expected true earnings. Denote by $E(X,p)$ the expected earnings in the non-shock state when auditor quality is poor but the company reports $X_0 = X_H$. It is straightforward to show that:

$$\begin{aligned} E(X, p) &= \text{Prob.}(X = X_H | X_H \text{ is reported}) X_H + \text{Prob.}(X = X_L | X_H \text{ is reported}) X_L \\ &= \frac{2p}{1+p} X_H + \frac{1-p}{1+p} X_L. \end{aligned} \tag{1}$$

Note that $E(X,p) < X_H$, which implies that equity is underpriced for companies with $X = X_H$. Namely, if the true earnings are $X = X_H$, then the equity will be undervalued if other companies with $X = X_L$ also issue equity when reported earnings are X_H (because of successful manipulation). This is true in any equilibrium because for $X = X_L$, the equity is, at worst, correctly priced and therefore equity issuance dominates debt issuance because of the expected financial distress cost associated with debt financing. In addition, the mispricing cost associated with equity issuance for $X = X_H$ will be higher the larger is the amount of external financing needed, i.e., the larger is the scale of the project. Thus, there exist parameter values for which the smaller scale project I_1 is financed with equity because the mispricing loss is not larger than the financial distress cost associated with debt financing, but the larger scale project I_2 is financed with debt because of the larger mispricing cost. A formal statement of the equilibrium is given below.

Proposition 1: (i) Companies with good-quality auditors will finance both I_1 and I_2 with equity.

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[4] If the company uses its internal funds as part of the financing for the project, then it needs to raise less than I. This, however, raises the possibility that the amount raised will reveal the true cash flow. To avoid this possibility, we can assume that either (1) the difference between X_H and X_L mainly comes from accruals, or (2) the actual scale of investment is a random draw from two possible distributions, one with a mean I_1 and the other with a mean I_2. The former assumption requires that no change be made in our analysis except for a redefinition of the funds raised as amounts net of X, whereas the latter requires that very minor changes be made in the analysis.

(ii) For companies with poor-quality auditors and true earnings of $X = X_L$:
  a. those reporting $X_0 = X_H$ because accounting manipulation is successful will issue equity to finance both I_1 and I_2; and
  b. those reporting $X_0 = X_L$ (because accounting manipulation is unsuccessful) will issue equity to finance both I_1 and I_2.

(iii) For companies with poor-quality auditors and true earnings of $X = X_H$, the conditions for issuance can be stated as follows:

Fix $p = p^*$. Suppose that I_1 satisfies[5]

$$(1 - \delta)c > \frac{X_H + I_1 + V}{E(X, p^*) + I_1 + V} I_1 - I_1. \tag{2}$$

We then have the following results:

a. The company issues equity for $I = I_1$. Further, there exists a value $I_2(p^*)$, where $I_2(p^*) > I_1$, such that for $I = I_2 \geq I_2(p^*)$, the company issues debt.
b. Fix I_2 at the value $I_2(p^*)$. Then, there exists $p' > p^*$ such that the company prefers equity to debt issuance for both $I = I_1$ and $I = I_2$.

Part (i) follows from the fact that for companies with good-quality auditors, there is no mispricing of equity. Thus, by issuing equity, the company can avoid the financial distress costs associated with debt. Part (ii) follows because companies with low reported earnings (and poor auditor quality) must have low actual earnings. As equity is correctly priced for such companies, they will not issue debt either. Finally, part (iii) considers issuance decisions when true earnings X (in the non-shock state) are high for a company with poor auditor quality. Appendix A provides a formal proof of part (iii). Reported earnings X_0 are high in this case; however, as the market does not know whether reported earnings are high because true earnings are high or because the poor-quality auditor has failed to detect misreporting, equity will be mispriced. If the issue size is large, then the mispricing cost of equity issuance will exceed the financial distress costs associated with debt. However, if economic conditions are good (i.e., p is high), then mispricing is smaller and firms may issue equity to avoid financial distress costs.

Endogeneity of Auditor Choice

So far, we have not discussed how companies and auditors are matched. We now show why some companies might prefer low-quality auditors while others might prefer high-quality ones. We will assume that high-quality auditors are more expensive than low-quality ones: without any loss of generality, assume that the fee for a high-quality auditor is f while that for a low-quality auditor is 0.

In terms of our model, a high-quality auditor adds value only when the scale of investment is $I = I_2$, when true earnings are $X = X_H$, and when p is not too high (as in part (iii)a of Proposition 1). This is because, under these conditions, the cost associated with equity mispricing is sufficiently high that companies with low-quality auditors have to issue debt and experience the expected financial distress costs, which amount to $(1 - \delta)c$. Thus, the benefit of a high-quality auditor is positively related to the probability $1 - q$ of the state $I = I_2$ occurring, and is given by $(1 - q)(1 - \delta)c$. Therefore, it is optimal to choose a high-quality auditor if and only if:

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[5] Note that for a sufficiently small I_1, this condition holds.

$$(1 - q)(1 - \delta)c > f.$$

Companies with sufficiently low q will therefore choose high-quality auditors, while those with higher q will choose low-quality auditors. Importantly, note that q is not a parameter that enters Proposition 1 and, therefore, does not affect the main results.

Empirical Implications

To interpret our results, it is useful to fix the exogenous probability of $I = I_1$ as q_i, where $0 < q_i < 1$, and $i = G, B$ denotes the auditor quality (*Good* or *Bad*). We assume that $q_B \geq q_G$.[6] The first empirical implication of Proposition 1 is as follows.

Empirical Implication 1: (i) Companies with poor-quality auditors will issue equity (as opposed to debt) less frequently than will those with good-quality auditors.
(ii) The average size of equity issues will be smaller for companies with poor-quality auditors than for those with good-quality auditors.

When I_1 satisfies condition (2) and $I_2 \geq I_2(p^*)$, we have companies with good-quality auditors always issuing equity but companies with poor-quality auditors issuing debt for $I = I_2$ (part (iii)a of Proposition 1) when $X = X_H$. Thus, the probability of equity issuance for companies with poor-quality auditors is $(1 - p) + pq_B < 1$, which corresponds to part (i) of Empirical Implication 1. The average size of equity issues for the companies with poor-quality auditors, conditional on equity issuance, is $(pq_BI_1 + (1 - p)(q_BI_1 + (1 - q_B)I_2))/(pq_B + (1 - p))$; this can be shown to be less than that for companies with good-quality auditors, given by $q_GI_1 + (1 - q_G)I_2$. Thus, the second part of the implication follows.[7]

Our model also has the following implications of the impact of market conditions on financing decisions.

Empirical Implication 2: (i) The probability of debt (as opposed to equity) issuance (for all companies combined) will decrease after good stock market performance.
(ii) The average size of equity issues (for all companies combined) will increase after good stock price performance.
(iii) The gap in the average size of equity issues between companies that have good-quality auditors and those that have poor-quality auditors will narrow after good stock price performance.

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[6] As the discussion above on auditor choice suggests, firms with lower q benefit more from the choice of a higher quality auditor.

[7] Notice that we can write: $(pq_BI_1 + (1 - p)(q_BI_1 + (1 - q_B)I_2))/(pq_B + (1 - p)) = (q_BI_1 + (1 - p)(1 - q_B)I_2)/(q_B + (1 - p)(1 - q_B))$. Since $q_B \geq q_G$, we have $1 - q_G \geq 1 - q_B > ((1 - p)(1 - (q_B))/(q_B + (1 - p)(1 - q_B))$. Because $I_2 > I_1$, we have $q_GI_1 + (1 - q_G)I_2 > (q_BI_1 + (1 - p)(1 - q_B)I_2)/(q_B + (1 - p)(1 - q_B))$.

To understand how these implications are derived, we can examine the effect of p. Part (iii)b of Proposition 1 shows two things. First, it shows that when times are good (i.e., p is high), companies with poor-quality auditors issue equity more often than when times are bad (with a probability of 1 when times are good compared with a probability $(1 - p) + pq_B$ when times are bad). The underlying intuition is that when market conditions are good, high-type companies are less undervalued because of the reduced information asymmetry. This corresponds to part (i) of Empirical Implication 2 and is consistent with the idea that the probability of debt issuance is negatively related to run-ups in stock prices (which would be associated with increases in p). Second, when p is sufficiently high, the average size of an issuance by companies with poor-quality auditors is larger compared with a situation in which p is not so high. This occurs because the mispricing faced by companies with high earnings but poor auditor quality is less when market conditions are good, which allows these companies to finance bigger projects with equity as well (and avoid potential financial distress costs from issuing debt). The average size of equity issues when p is high by companies with poor-quality auditors is $q_BI_1 + (1 - q_B)I_2$; this exceeds the average size of $\frac{pq_BI_1 + (1 - p)(q_BI_1 + (1 - q_B)I_2)}{pq_B + (1 - p)}$ when p is low. This result corresponds to part (ii) of Empirical Implication 2. Finally, it also follows that the *difference* in the average size of equity issuance between companies with good-quality auditors and those with poor-quality auditors will be less during good times compared with bad times, consistent with the third part of the implication.[8]

Relation to Prior Studies

The basic intuition behind our model is similar to that behind the model of Choe et al. (1993); that is, during good times, adverse-selection costs will be lower. This occurs because the cash flows from a company's assets in place have a publicly observable component that is related to general economic conditions and to a component that is private information to the company's insiders, as noted in Myers and Majluf (1984). This publicly available component is relatively more important during good times, thereby reducing the adverse-selection costs of equity issuance. A similar effect is present in our context: if the probability of the *High* state is higher during good times, then the *High*-type company with $X = X_H$ is less underpriced even when the market is unable to distinguish company types and equity issuance is less costly. However, our model also differs from that of Choe et al. (1993). Whereas their study derives implications for the aggregate volume of equity issues and the market reaction to equity issuance announcements as a function of economic conditions, we derive implications for the size of equity issuance (in addition to the probability of equity issuance) as a function of market conditions. These implications are new to the literature. Moreover, we show how auditor quality interacts with market conditions to affect the probability and size of equity issues across companies.

We articulate the main arguments in terms of a model of time-varying adverse selection. In this approach, investors are fully rational and there is no price bubble. There are also numerous models based on irrational investor exuberance in which companies have incentives to take advantage of temporary mispricing of equity. For example, a well-known body of work links the long-term underperformance of companies engaged in seasoned equity issues or initial public offerings to earnings manipulation.[9] If better-quality auditors limit the potential for companies to manipulate earnings, then there is greater potential for favorable misvaluation for companies with poor-quality auditors when stock prices are high. If this is true, then these companies may engage in equity financing more actively when prices are high. However, our tests are not designed to distinguish between such models and those based on time-varying adverse selection.

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[8] To see this, notice that companies with good-quality auditors issue equity in both states at all times and hence the average size of equity issuance is $q_GI_1 + (1 - q_G)I_2$ irrespective of p.

Testable Hypotheses

The empirical implications of our model generate several testable hypotheses. We discuss these here and outline our estimation methodology.

Audit Quality and the Debt-Equity Choice

Empirical Implication 1 leads to our first hypothesis, as follows.

Testable Hypothesis 1: Companies with Big 6 auditors are more likely to issue equity as opposed to debt than are companies with non-Big 6 auditors.

To test H1, we estimate the following probit model, which is similar in spirit to that of Hovakimian et al. (2001):

$$P[Dissue_{i,t} = 1] = F(a_0 + a_1 BigSix_{i,t-1} + a_2 DevTgt_{i,t-1} + \gamma C_{i,t-1}) \tag{3}$$

where P stands for the probability of debt being issued, F denotes the normal cumulative distribution function, and $Dissue_{i,t}$ takes a value of 1 if the net debt issued by company i in fiscal year t constitutes more than 5 percent of its total assets, and 0 if the net equity issued exceeds 5 percent of its total assets. Only issue years in which the company issues net debt or equity exceeding 5 percent of its book value of assets are considered; years in which both are issued or neither is above the 5 percent cutoff are not included in the model. *BigSix* is an indicator variable that takes a value of 1 if the company is audited by a Big 6 firm, and 0 otherwise.[10] The coefficient on *BigSix*, a_1, is expected to be negative. C denotes a set of predetermined (one-period lagged) control variables that have been shown in the prior literature to influence a company's debt-equity choice (e.g., Frank and Goyal 2003). The interpretations of the control variables are presented in Section III, and the definitions of these variables are provided in Appendix B. In addition, we control for the deviation from the target leverage ratio (*DevTgt*) in the estimation because trade-off theory suggests that the change in the debt ratio will also be related to whether the company is above or below the target. The deviation from the target leverage ratio is measured as the difference between the leverage ratio (lagged one year) and the target debt ratio. We report the target leverage ratio estimation in Appendix C, where we regress the leverage ratio on the control variables, C. The target leverage ratio is the predicted value from this cross-sectional leverage regression.

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[9] See Teoh et al. (1998a, 1998b) for evidence of aggressive earnings management prior to equity issuance and subsequent return underperformance, and Jain and Kini (1994) and Loughran and Ritter (1997) for evidence that earnings deteriorate after equity issuance.

[10] *BigSix* takes a value of 1 if the audit code (Compustat item 149) is between 1 and 8, and 0 otherwise.

Audit Quality and the Size of Equity Issuance

The first and second empirical implications lead to the following testable hypothesis concerning the size of equity issues:

Testable Hypothesis 2: (i) The size of equity issues will be larger for companies with Big 6 auditors;
(ii) the size of equity issues will be larger following good recent stock price performance; and
(iii) the gap in the average equity issue size between companies with and those without Big 6 auditors will be smaller subsequent to good recent stock price performance.

To test H2, we regress the size of equity issue on past stock returns (*PRTN*), controlling for different company characteristics. We expect *BigSix* to be associated with larger issuances. Consistent with the prior literature, we also expect that past returns will have a positive effect on issuance size. We then introduce an interaction between past returns and the Big 6 dummy. We expect that the interaction will have a negative effect (i.e., the effect will not be as strong for companies audited by Big 6 firms). Specifically, we estimate the following model for companies issuing equity:

$$\begin{aligned} Esize_{i,t} = {} & b_0 + b_1 PRTN_{i,t-1} + b_2 BigSix_{i,t-1} + b_3 PRTN_{i,t-1} \times BigSix_{i,t-1} \\ & + b_4 DevTgt_{i,t-1} + \gamma C_{i,t-1} + \varepsilon_{i,t} \end{aligned} \tag{4}$$

where $Esize_{i,t}$ denotes the size of the net equity issue of company i in fiscal year t, scaled by the book value of assets at the beginning of the fiscal year; *PRTN* denotes the cumulative monthly stock return for the fiscal year; and C is the set of control variables used in Equation (1), which are expected to influence the size of the equity issuance as well. The deviation of the actual debt ratio from an estimated target (*DevTgt*) is included to capture a company's motive to choose the size of its equity issuance to adjust the leverage ratio toward its target. We expect coefficients b_1 and b_2 to be positive and, more importantly, coefficient b_3 to be negative.

As the company's decision to issue equity is an endogenous choice, and Equation (4) is estimated only over the company-years in which an equity issuance decision is observed, the OLS estimates could be biased. To address this selection issue, we re-estimate Equation (4) as part of a Heckman selection model. We posit that companies will issue equity if:

$$\rho Z_{i,t-1} + u_{i,t} > 0 \tag{5}$$

where Z is a vector of predetermined company-specific variables, and u is a standard normally distributed error term. We allow for the possibility that u and ε in Equation (4) are correlated; if the correlation coefficient is nonzero, then standard regression techniques applied to Equation (4) will result in biased estimates. Heckman's maximum likelihood estimation provides consistent, asymptotically efficient parameter estimates. The vector Z includes variables that are likely to affect the company's requirement for external funds, as well as those that affect the choice of equity *vis-à-vis* debt financing. These variables include proxies for the company's investment needs (in the current period and in the next year), proxies for internally available funds, variables that are relevant for the determination of the company's cash-holding targets, and variables that are relevant for the determination of its debt capacity. Appendix B gives a complete list of all of the variables that vector Z includes.

III. DATA, VARIABLES, AND SUMMARY STATISTICS

Data

Our main sample consists of companies listed in the Compustat Industrial Annual files at any point between 1985 and 2005. We start our sample in 1985 because we control for the marginal tax rate, analyst coverage, and debt rating in all multivariate analysis, and 1985 is the first year that all of these variables are commonly available. As a robustness check, we extend our sampling period to 1974 (when data on auditor size became available) and exclude the control variables for which we cannot obtain data. Our results (untabulated) are qualitatively similar. The data on stock prices and returns are retrieved from the Center for Research in Security Prices (CRSP) files. Following the practice of earlier studies, we exclude financial, insurance, and real estate companies (i.e., SIC codes 6000–6900) because their capital structures are likely to be significantly different from the industrial companies in our sample. We also exclude regulated utilities (SIC codes 4900–4999) because their financial policies are governed by regulatory requirements. To ensure that our results are not driven by extremely small companies, we also exclude observations if the company's assets are less than $10 million. Companies with missing book values of assets or those that have fewer than five consecutive years of data are not included because several key variables of interest are measured over a five-year period. We define equity and debt issues using cash flow statement data (see Appendix B).[11] All variables have been winsorized at the top and bottom 0.5 percent of their distributions. Dollar values are adjusted to the 2000 dollar value using a GDP deflator.

Control Variables

As mentioned in Section II, we control for a set of variables that have been shown in the prior literature to influence a company's capital structure.

We include the log of the book value of assets as a proxy for company size. We also control for company maturity by including the log of the company's age plus 1. We expect that the incentive to take on debt increases with the company's marginal tax rate because of the tax deductibility of interest expenses. Thus, we include the simulated corporate marginal tax rate based on the operating income after interest expenses (Graham 1996). We also control for liquidity by including share turnover. Finally, companies having more tangible assets are expected to support more debt as these assets can be pledged as collateral. The net PPE-to-asset ratio measures the tangibility of the firm's assets. Research and development (*R&D*) expenses scaled by sales can proxy for a variety of company characteristics, such as the uniqueness of the product (Titman 1984), information asymmetry, or growth potential. We also include an *R&D* indicator variable that equals 1 if *R&D* expenses are missing, and 0 otherwise. To control for the risk and financial constraints faced by companies, we include Altman's (1968) unleveraged Z-score, stock return volatility, and earnings volatility.

Chang et al. (2006) find that companies with greater analyst coverage have lower leverage and weaker incentives to take advantage of favorable conditions in the equity market. Thus, for each year, we set the number of analysts following a company as the maximum number of analysts who make annual earnings forecasts in any month over a 12-month period. Faulkender and Petersen (2006) document that companies that have access to public bond markets, as measured by having a debt rating, take on more debt. To capture a company's access to bond markets, we include a debt-rating indicator variable that equals 1 if a company has a debt rating assigned by Standard & Poor's, and 0 otherwise.

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[11] Our definition of debt issues does not include the increase in accounts payable, the increase in income taxes payable, and the increase in other current liabilities. As a robustness check, we follow Baker and Wurgler (2002) and define equity and debt issues using balance sheet data where debt issues are defined as the change in total liability plus the change in preferred stock minus the change in deferred taxes and the change in convertible bonds. Our main results remain the same under this alternative definition.

Various capital structure theories suggest that leverage is related to profitability. For example, it could be argued that profitability is a proxy for debt capacity, and there should therefore be a positive association between profitability and leverage. We use return on assets (ROA) as a proxy for profitability. We expect that companies will be more likely to issue equity when their stock performance has been good. This is consistent with both the equity market timing and adverse-selection arguments, as companies are less likely to be undervalued during such periods. To measure a company's past stock performance, we use, in alternative specifications, two cumulative stock returns, which are obtained by compounding monthly stock returns from year $t-2$ to $t-1$ and from year $t-5$ to t, respectively. We employ the market-to-book ratio as a control for growth opportunities. Most studies document a negative relation between the market-to-book ratio and leverage ratio, possibly because growth companies have greater incentives to avoid debt overhang problems. We control for the corporate payout policy because it may be related to capital structure decisions. If current profits are positively correlated with future profits, then companies expecting higher future profits may pay more dividends and have lower debt ratios by retiring debt. However, to the extent that higher future profits reflect higher debt capacities, companies that pay more dividends may choose higher debt ratios.

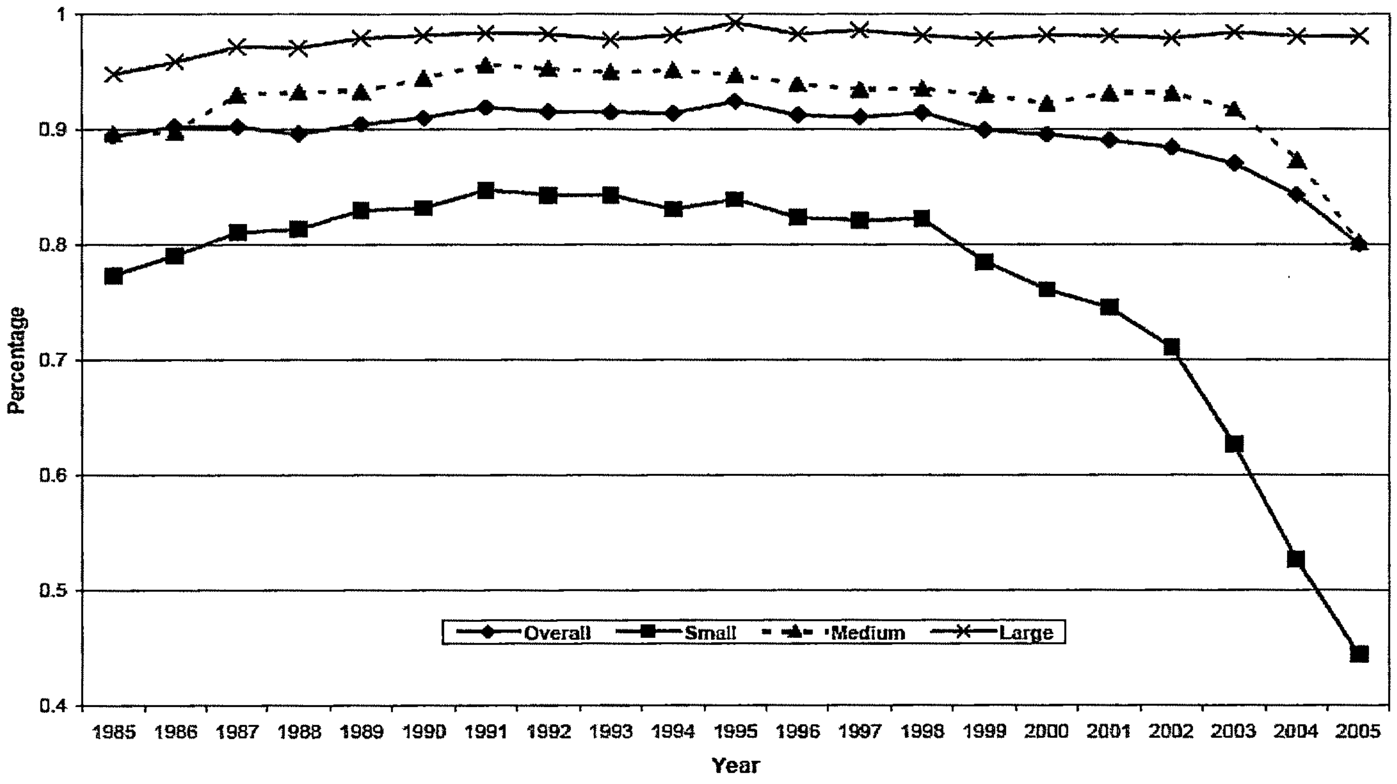
Summary Statistics

Figure 1 reports the evolution of the percentage of companies audited by Big 6 firms. We split the sample evenly into three groups according to the book value of assets at the beginning of the fiscal year. Group 1 consists of the smallest companies and group 3 includes the largest ones.

Table 1 shows that, in the overall sample, 89.7 percent of the companies are audited by Big 6 auditors. Noticeably, large companies are more likely to be audited by Big 6 firms; the percentage of companies audited by Big 6 firms increases across the three subgroups based on size (from 78.6 percent for small companies to 97.9 percent for large ones). Overall, the market share of the Big 6 varies over time, ranging from a low of 80.1 percent of companies in 2005 to a high of 92.4 percent in 1995. Interestingly, we observe a decline in recent years in the percentage of small and medium companies audited by Big 6 firms.

Table 1 reports the descriptive statistics for the overall sample, two groups of companies sorted on the basis of auditor quality, and three groups sorted on the basis of company size. When we compare companies audited by Big 6 firms with those audited by smaller firms, we observe that companies audited by Big 6 firms are larger, more profitable, and less leveraged. They also have more tangible assets and a greater likelihood of having analyst coverage and credit ratings. However, differences in analyst coverage and bond ratings are also driven to a large extent by differences in company size. For the group of small companies, the median number of analysts (not tabulated) is 0 compared with 10 for the group of large companies, and only 0.6 percent of small companies have their debt rated by Standard & Poor's. In these companies, auditors are likely to play a key role as there are no additional outside informational intermediaries between managers and investors. This finding contrasts with that of Chang et al. (2006), who investigated the effect of variation

FIGURE 1
Percentage of Companies Audited by Big 6 Firms



The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. Financial, insurance, and real estate companies, regulated utilities, and companies with assets less than $10 million are excluded. Also excluded are companies with missing book values of assets. The overall sample is evenly partitioned into three groups based on the book value of assets at the beginning of the fiscal year. The figure reports the time trend of the percentage of firms audited by Big 6 auditors. Firms are defined as audited by Big 6 firms if their Compustat auditor code (Compustat data item 149) is between 1 and 8.

TABLE 1
Summary Statistics

Panel A: Mean Values Of Firm-Specific Variables

	Overall Sample	Audit Quality Grouping			Company Size Grouping		
		Big 6	Non-Big 6	t-statistic	Small	Medium	Large
Age	21.8	22.2	18.4	18.7***	16.5	20.2	28.8
Book Value of Assets	2,425	2,650	444	14.4***	46.5	292	6,938
Tangibility	0.31	0.32	0.28	12.8***	0.26	0.31	0.38
Leverage ratio (Lev)	0.19	0.19	0.21	−5.7***	0.18	0.20	0.20
Return on Assets	9.6%	10.4%	3.0%	−8.4***	2.2%	12.2%	14.4%
Market-to-Book Ratio	1.77	1.79	1.62	4.7***	1.84	1.68	1.79

Panel B: Summary Statistics of Debt/Equity Issues

	Overall Sample	Audit Quality Grouping			Company Size Grouping		
		Big 6	Non-Big 6	t-statistic	Small	Medium	Large
Percentage of companies issuing debt	23.3%	23.3%	23.4%	−0.4	20.2%	23.3%	26.4%
Percentage of companies issuing equity	7.4%	7.1%	8.5%	3.1***	10.6%	7.5%	4.2%
Average size of debt issues	0.19	0.18	0.21	−4.8***	0.20	0.20	0.16
Average size of equity issues	0.31	0.30	0.32	−1.1	0.37	0.30	0.14
Percentage of companies having rated debt	29.1%	31.8%	5.0%	36.3***	0.6%	15.4%	71.3%
Percentage of companies covered by analysts	60.4%	63.5%	33.5%	37.8***	37.3%	70.1%	74.0%
Percentage of companies audited by Big 6 firms	89.7%	100%	0%		78.6%	92.7%	97.9%
Number of company-years	39,837	35,757	4,080		13,279	13,279	13,279

(*continued on next page*)

TABLE 1 (continued)

The two-tailed significance levels of the t-statistics are marked with *, **, and *** for differences significant at the 10 percent, 5 percent, and 1 percent levels, respectively. Dollar figures are in millions.

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The overall sample is evenly partitioned into three groups based on the book value of assets at the beginning of the fiscal year.

Net equity and debt issued are computed using Compustat's cash flow statement data. Equity issues equal the sale of common and preferred stock minus the purchase of common and preferred stock. Debt issues equal long-term debt issuance minus long-term debt reduction plus changes in current debt. Companies are defined as issuing debt (equity) when the net debt (equity) issued divided by total assets exceeds 5 percent. Cases where companies issue both debt and equity in a given fiscal year are not counted. The size of debt and equity issues is scaled by total assets at the beginning of the fiscal year. A firm has rated debt if either senior or subordinated debt is rated by Standard & Poor's. A firm is covered by analysts if the number of analysts is greater than zero. The number of analysts is the maximum number of analysts making annual earnings forecasts any month over a 12-month period.

The t-statistic tests the difference in the mean (average) levels of the variables between companies audited by Big 6 and those audited by non-Big 6 firms.

Variable Definitions:

Age = number of years since the firm entered Compustat;
Book Value of Assets = Compustat item #6;
Tangibility = net PPE-to-asset ratio;
Leverage Ratio (*Lev*) = total debt divided by the sum of total debt and the product of the number of the shares outstanding and the closing stock price at the end of the fiscal year;
Market-to-Book Ratio = (market value of equity + book value of debt)/book value of assets; and
Return on Assets = income before depreciation and amortization divided by the book value of assets.

in analyst coverage on financial decisions. In their setting, most of the variation occurs among companies that are large enough to attract some coverage. In our setting, most of the variation occurs among small companies. Table 1 also shows that companies audited by Big 6 firms have less debt in their capital structure than do those audited by small auditors.

Table 2 reports a correlation table. Univariate correlations also indicate that companies audited by Big 6 auditors tend to have less debt in their capital structure. In addition, Table 2 indicates significant positive relations among company size, being audited by a Big 6 firm, analyst coverage, and debt rating. Other correlations are generally low, which suggests that multicollinearity is not a major problem in our setting.[12]

Determinants of Auditor Quality

Table 3 reports the results of a probit regression in which the dependent variable is *BigSix.* This analysis provides the basis for our control for the possible endogeneity of auditor quality discussed in Section IV. In selecting control variables, we start by introducing different company characteristics proposed by previous studies: size, age, stock price, asset turnover, asset growth (Willenborg 1999), the Herfindahl index, current ratio (Weber and Willenborg 2003), and whether the company discloses any R&D. We also control for ROA (Chaney et al. 2004). We include several measures for risk: a dummy variable for companies working in industrial sectors in which litigation risk is high (Hogan 1997), return and earnings volatility, the industry's median debt ratio, and bankruptcy risk (Z-score). We also control for different measures of the complexity of the auditing assignment: the number of segments, R&D intensity, and asset tangibility. The results indicate that larger companies with higher asset turnover, higher stock price, higher ROA, lower litigation risk, lower earnings volatility, lower growth, and higher tangibility are more likely to be audited by Big 6 firms. Note, however, that our main objective here is not to identify a demand or supply function for auditor quality, but rather to identify mere correlates with auditor quality to obtain a basis for our procedures that control for the endogeneity discussed in Section IV.

IV. MAIN EMPIRICAL TESTS AND RESULTS

Here, we present the empirical results for our two main hypotheses.

Auditor Quality and Debt-Equity Choice

Our first hypothesis predicts that companies with Big 6 auditors are more likely to issue equity as opposed to debt than are those with non-Big 6 auditors. To test this hypothesis, we examine whether auditor size affect the debt-equity choices of companies in a given year. Following Hovakimian et al. (2001), we estimate a regression Equation (3) using three empirical approaches. We start with a simple maximum likelihood approach. We allow for clustering of observations of a company to adjust the standard errors for serial correlation and correct the standard errors for heteroscedasticity. However, these approaches may lead to biased coefficients endogeneity in the selection of the auditor. We cannot address this problem using the traditional 2SLS procedure because the dependent variable in the second stage is a binary variable. Instead, we use two additional approaches as follows.

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[12] We calculate the variance inflation factors for our OLS regressions in 5 and 6. The average VIF is below 2 and no individual factor is greater than 10, a conventional level for significant multicollinearity.

TABLE 2
Correlation Coefficients among Key Variables

	BigSix	*Lev*	*Log (Assets)*	*Tangibility*	*Return on Assets*	*Past Stock Return*	*Market-to-Book*	*Z-Score*	*Marginal Tax Rate*	*Analyst Following*
Leverage Ratio (Lev)	−0.03***									
Log (Assets)	0.27***	0.04***								
Tangibility	0.06***	0.25***	0.23***							
Return on Assets	0.04***	−0.02***	0.11***	0.04***						
Past Stock Return	−0.00	−0.16***	0.00	−0.02***	0.05***					
Market-to-Book Ratio	0.02***	−0.24***	−0.02***	−0.10***	−0.03***	0.19***				
Unleveraged Z-Score	0.03***	−0.03***	0.10***	−0.01**	0.63***	0.04***	−0.57***			
Marginal Tax Rate	0.09***	−0.02***	0.35***	0.10***	0.19***	0.06***	−0.08***	0.19***		
Analyst Following	0.16***	−0.14***	0.58***	0.12***	0.07***	0.00	0.11***	0.04***	0.21***	
Debt Rating	0.18***	0.18***	0.66***	0.20***	0.05***	−0.01**	−0.02***	0.02***	0.18***	0.43***

Correlation coefficients that are significant at the 1 percent, 5 percent, and 10 percent level are marked with ***, **, and *, respectively.
The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The pairwise Pearson correlation coefficients among variables are reported.

Variable Definitions:

BigSix = 1 if a company chose a Big 6 Auditor, and 0 otherwise;
Leverage Ratio (Lev) = total debt divided by the sum of total debt and the product of the number of the shares outstanding and the closing stock price at the end of the fiscal year;
Log (Assets) = log value of the total book value of assets;
Tangibility = net PPE-to-asset ratio;
Past Stock Return = compounded annual stock return over a 12-month period;
Return on Assets = income before depreciation and amortization divided by the book value of assets;
Market-to-Book Ratio = (market value of equity + book value of debt)/book value of assets;
Unleveraged Z-score = (3.3 × pretax income + sales + 1.4 × retained earnings + 1.2 × [current assets − current liabilities]/book value of assets);
Marginal Tax Rate = simulated marginal tax rate after interest expenses obtained from John Graham's website;
Analyst Following = maximum number of analysts making annual earnings forecasts any month over a 12-month period; and
Debt Rating = dummy equal to 1 if either senior or subordinated debt is rated by Standard & Poor's, and 0 otherwise.

TABLE 3
Determinants of Auditor Quality (Probit Model)

Auditor Quality (*BigSix*)	Coef.	z-stat.
Log (Assets)	0.339***	(14.4)
Log (Age)	−0.125**	(−2.5)
Log (Stock Price)	0.116***	(3.8)
Asset Turnover	0.092**	(2.4)
Return on Assets	0.270*	(1.7)
Industry Median Leverage Ratio	−0.302	(−1.4)
Unleveraged Z-Score	−0.035**	(−2.2)
Stock Return Volatility	0.729	(1.0)
Earning Volatility	−0.001***	(−4.9)
Litigation Risk Dummy	−0.221*	(−1.9)
Herfindahl Index	−0.106	(−0.7)
Market-to-Book Ratio	0.024*	(1.8)
Asset Growth	−0.188***	(−5.0)
Number of Industrial Segments	−0.037***	(−2.7)
R&D-to-Sales Ratio	0.132***	(3.4)
R&D Dummy	−0.106**	(−2.1)
Tangibility	0.435***	(2.6)
Current Ratio	0.298**	(2.0)
Pseudo-R^2/Firm-years	0.15/39,837	

Coefficients that are significant at the 10 percent, 5 percent, and 1 percent level are marked with *, **, and ***, respectively.

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The dependent variable is the Big 6 dummy (*BigSix*), which equals 1 if a firm chose a Big 6 Auditor, and 0 otherwise.

All explanatory variables are measured one year before a firm's auditor quality is observed. Constant terms and year dummies are included in the regressions but not reported. The z-statistics in parentheses are calculated from the Huber/White/sandwich heteroscedastic consistent errors, which are also corrected for correlation across observations for a given firm.

Variable Definitions:

Age = number of years since a firm entered the data set;
Industry Median Leverage Ratio = computed by the three-digit SIC code and by year;
Asset Growth = change in the log of total assets;
Litigation Risk Dummy = 1 if the firm is in a high litigation risk industry, and 0 otherwise;
Herfindahl Index = computed by summing the squared market shares within each three-digit SIC industry;
Market-to-Book Ratio = (market value of equity + book value of debt)/book value of assets;
Stock Price = median monthly closing prices over a 12-month period;
Number of Industrial Segments = number of business segments obtained from Compustat segment data;
R&D to Sales ratio = R&D expenses divided by net sales;
R&D Dummy = 1 if R&D expenses are missing;
Return on Assets = income before depreciation and amortization divided by total assets;
Tangibility = net PPE-to-asset ratio;
Asset Turnover = sales divided by total assets;
Current Ratio = current assets divided by total assets;
Stock Return Volatility = standard deviation of daily stock returns calculated for each firm each year; and
Earning Volatility = standard deviation of the EBIT-to-asset ratios over a firm's entire life in Compustat.

First, we predict the likelihood of the company selecting a Big 6 auditor in the first stage using the specification in Table 3, and use this predicted value in the second stage. To further mitigate the effect of endogeneity, auditor choice is lagged one period and is first predicted based on company-specific variables that are lagged two periods. We bootstrap the system 500 times to obtain the 95 percent confidence intervals. In addition, we follow a technique suggested in the prior econometric literature (for example, Wooldridge 2002), which involves using the predicted value of auditor quality as an instrument in the instrumental regressions. This approach has three steps. In the first step, we predict auditor quality as in Table 3. In steps 2 and 3, we use a standard instrumental approach, with the predicted value obtained from step 1 as an instrument. In our subsequent analysis we interact *BigSix* with past stock returns, we follow Wooldridge (2002) and used the demeaned value of past returns.[13] Wooldridge (2002) indicates that, under fairly general conditions, this procedure provides efficient estimations. In addition, we are using the fitted probability of auditor quality as an instrument, the effect of misspecification in auditor quality model is mitigated. We also correct the standard errors for heteroscedasticity and serial correlation.

The results in Table 4 from the three estimation procedures are very similar. They all indicate that *BigSix* is significantly associated with a lower likelihood of issuing debt as opposed to equity. The z-statistic equals −2.5 in Column I and −4.7 in Column III of Table 4. The bootstrapped 95 percent confidence interval, reported in Column II, indicates that the coefficient of auditor quality is significantly different from zero.[14] The economic magnitude is such that the likelihood of issuing equity the average company in the sample would be 3.5 percent higher if it were audited by a Big 6 rather than a small auditing firm.[15] By comparison, increasing the company's size by one standard deviation changes the probability of issuing equity by 2.6 percent. By assuming that a company chooses to issue debt (equity) if the estimated probability is greater (lower) than 50 percent, our model correctly predicts the debt-equity choice 80 percent of the time. The control variables generally have signs consistent with prior literature (e.g., Titman and Wessels 1988; Frank and Goyal 2003). For example, companies that are more mature, more profitable, less risky or have a higher marginal tax rate are more likely to issue debt. It is important to note that the auditor effect exists above and beyond the effect of other financial intermediaries such financial analysts or debt-rating agencies.

Auditor Quality, Size of Issuance, and Market Conditions

Our second hypothesis is that all companies will issue more equity when market conditions are good and those audited by non-Big 6 auditors are more likely to take advantage of favorable market conditions. Put differently, the impact of auditor quality on the size of equity issuance will be smaller when market conditions improve. To test this hypothesis, we estimate Equation (4). Specifically, we regress the size of equity issues scaled by the book value of assets on the control variables and the compounded monthly stock returns over the previous fiscal year, as well as the latter's interaction with the Big 6 dummy.

Table 5 presents the results obtained from the estimation of Equation (4). We first estimate this model in Column I of Table 5 using the standard OLS approach and correcting the standard errors for heteroscedasticity and serial correlation. However, endogeneity is

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[13] See Wooldridge (2002, Section 18.5.2).

[14] Untabulated results show that our results hold when we use a bivariate probit procedure specification to simultaneously model the debt-equity choice and the decision to choose a Big 6 auditor.

[15] To estimate this difference, we set the value of all control variables to their means in the sample. We estimate the likelihood of the company issuing equity, assuming that *BigSix* equals 0. We then estimate the same probability if *BigSix* equals 1. The difference of the two probabilities is equal to 3.5 percent.

TABLE 4
Debt-Equity Choices and Audit Size (Probit Model)

	(I) Probit		(II) Two-Stage Bootstrap		(III) Two-Stage Wooldridge	
Dissue	**Coef.**	**z-stat.**	**Coef.**	**95% Interval**	**Coef.**	**z-stat.**
BigSix	−0.127**	(−2.5)	−0.164	[−0.305, −0.023]	−1.318***	(−4.7)
Size of Total Financing	0.173***	(4.1)	0.181	[0.084, 0.277]	0.191***	(4.9)
Deviation from Target	0.371***	(3.1)	0.407	[0.166, 0.648]	0.279***	(2.6)
Log (Assets)	0.089***	(5.8)	0.117	[0.020, 0.214]	0.147***	(7.4)
Log (Age)	0.170***	(5.2)	0.165	[0.090, 0.240]	0.141***	(4.6)
Tangibility	0.057	(0.7)	0.380	[0.198, 0.561]	0.094	(1.3)
Return on Assets	0.668***	(3.9)	0.482	[0.157, 0.807]	0.688***	(4.7)
Share Turnover	−101.4***	(−5.4)	−96.7	[−131.9, −61.4]	−107.4***	(−7.9)
Past Stock Return	−0.214***	(−10.5)	−0.164	[−0.205, −0.123]	−0.221***	(−11.6)
Market-to-Book Ratio	−0.087***	(−4.9)	−0.082	[−0.118, −0.044]	−0.078***	(−7.6)
R&D-to-Sales Ratio	−0.112***	(−3.3)	−0.103	[−0.185, −0.022]	−0.067**	(−2.3)
R&D Dummy	0.099***	(2.9)	0.057	[−0.025, 0.140]	0.058*	(1.7)
Unleveraged Z-Score	0.064***	(4.6)	0.075	[0.045, 0.104]	0.070***	(6.7)
Dividend-to-Assets Ratio	6.466**	(2.5)	6.722	[0.636, 12.80]	6.453***	(6.1)
Stock Return Volatility	0.798	(1.0)	1.069	[−0.678, 2.82]	−0.181	(−0.2)
Earning Volatility	−0.000	(−0.1)	−0.001	[−0.002, 0.001]	−0.002*	(−1.7)
Marginal Tax Rate	0.695***	(4.2)	0.751	[0.408, 1.095]	0.658***	(4.4)
Analyst Following	−0.012***	(−4.6)	−0.012	[−0.018, −0.007]	−0.012***	(−4.5)
Debt Rating	0.094**	(2.0)	0.086	[−0.007, 0.179]	0.109**	(2.5)
Pseudo-R^2/Firm-years	0.20/12,232		0.20/500 replications		0.16/12,232	

Coefficients that are significant at the 10 percent, 5 percent, and 1 percent level are marked with *, **, and ***, respectively.

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The dependent variable, *Dissue*, equals 1 if debt is issued, and 0 if equity is issued. Companies are defined as issuing debt (equity) when the net debt (equity) issued divided by total assets exceeds 5 percent. Cases where companies issue both debt and equity in a given fiscal year are omitted. The explanatory variables are predetermined and defined in Appendix B. Constant terms are also included in the regressions but not reported. The z-statistics in parentheses are calculated from the Huber/White/sandwich heteroscedastic consistent errors, which are also corrected for correlation across observations for a given firm. Column II reports the regression using a two-stage procedure. In the first stage, the likelihood of the firm selecting a Big 6 auditor is predicted based on firm-specific variables using the probit model in Table 3. In the second stage, debt-equity choice is regressed on the predicted auditor quality and other control variables. The 95 percent confidence intervals in brackets are calculated from 500 bootstrap replications of the two-step estimation based on resampling from the data set with replacement of clusters. The average pseudo-R^2 of 500 replications is reported. Column III reports the results from an instrumental variable approach, which is described in Wooldridge (2002).

again a source of concern for two reasons. The first is the auditor choice, which may not be random as we have already noted. The second is that we limit our sample in this test to companies that issue equity and this issuance is also endogenous. To address the first type of endogeneity, we employ the same two approaches as those used in Table 4. The results are presented in Columns II and III of Table 5. To address the second type, we estimate a Heckman (1979) selection model in which the issuance decision is endogenized in the first step. Variables included in the first stage are those that are likely to affect the

TABLE 5
Size of Equity Issue, Price Run-Ups, and Auditor Quality

	(I) OLS		(II) Two-Stage Bootstrap (Auditor Quality Endogenized)		(III) Two-Stage Wooldridge		(IV) Heckman (Equity Issuance Decision Endogenized)	
Size of Equity Issue (*Esize*)	**Coef.**	**t-stat.**	**Coef.**	**95% interval**	**Coef.**	**t-stat.**	**Coef.**	**z-stat.**
Past Stock Return (PRTN)	0.056***	(4.3)	0.074	[0.065, 0.083]	0.142***	(3.6)	0.024***	(2.6)
BigSix	0.084***	(4.0)	0.124	[0.096, 0.154]	0.501***	(3.7)	0.041**	(2.1)
BigSix × PRTN	−0.029**	(−2.3)	−0.038	[−0.043, −0.032]	−0.139***	(−3.7)	−0.017**	(−2.2)
Log (Assets)	−0.072***	(−12.0)	−0.099	[−0.110, −0.089]	−0.084***	(−9.4)	−0.028***	(−5.4)
Log (Age)	−0.001	(−0.1)	−0.002	[−0.010, 0.007]	0.001	(0.1)	0.069***	(5.7)
Tangibility	0.078***	(3.1)	0.072	[0.052, 0.094]	0.059**	(2.5)	−0.046	(−1.6)
Return on Assets	−0.391***	(−6.4)	−0.349	[−0.390, −0.308]	−0.258***	(−4.9)	−0.218***	(−3.7)
Market-to-Book Ratio	0.013***	(3.7)	0.021	[0.018, 0.024]	0.015***	(4.3)	0.003	(0.6)
Share Turnover	48.62***	(10.2)	43.91	[39.49, 48.31]	42.16***	(7.7)	28.38***	(5.2)
Unleveraged Z-Score	−0.008**	(−2.0)	−0.003	[−0.008, −0.002]	−0.009**	(−2.6)	0.019***	(3.7)
Dividend-to-Assets Ratio	−0.104	(−0.4)	−0.197	[−0.662, 0.268]	0.354	(0.9)	0.619	(1.4)
Stock Return Volatility	−2.010***	(−6.5)	−1.747	[−2.123, −1.371]	−0.982***	(−2.9)	−0.489	(−1.6)
Earning Volatility	0.075***	(3.3)	0.080	[0.063, 0.096]	0.083***	(2.8)	−0.014	(−0.1)
Deviation from Target Debt Ratio	0.051***	(3.4)	0.009	[−0.019, 0.038]	0.006	(0.1)	0.159***	(3.7)
Marginal Tax Rate	0.039	(0.6)	0.048	[0.011, 0.086]	0.018	(0.3)	0.178***	(3.6)
Analyst Following	−0.001*	(−1.7)	−0.002	[−0.003, −0.000]	−0.002**	(−2.4)	−0.006	(−0.4)
Debt Rating	0.023*	(1.8)	0.031	[0.012, 0.051]	0.051***	(3.7)	0.024***	(3.1)
[Log pseudo-likelihood] R^2/Firm-years	0.31/2,939		0.30/500 replications		0.16/2,939		[−8570.1]/39,837	

(continued on next page)

TABLE 5 (continued)

Coefficients that are significant at the 10 percent, 5 percent, and 1 percent level are marked with *, ** and ***, respectively.

The t (z)-statistics in parentheses are calculated from the Huber/White/sandwich heteroscedastic consistent errors, which are also corrected for correlation across observations for a given firm.

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The dependent variable (*Esize*) is the size of net equity issue scaled by the book value of assets at the beginning of the fiscal year. Only equity issues exceeding 5 percent of total assets are included in Columns I–III. The Heckman equation in Column V is estimated using the entire sample. Net equity issued is the sale of common stock minus the purchase of common stock. *Past Stock Return* (*PRTN*) is the compounded monthly stock return over the previous fiscal year. The control variables and variables in the selection equation (Column IV) are defined in Appendix B. Constant terms and year dummies are included in both the regression equation and the selection equation, but not reported. Column I presents the OLS estimates. Column II reports the results when auditor quality is endogenized using the Two-Stage bootstrap approach. The average R^2 of 500 replications is reported. Column III reports the results from an instrumental variable approach, which is described in Wooldridge (2002). Column IV endogenizes the issuance decision.

company's requirement for external funds, as well as the choice of equity *vis-à-vis* debt financing. Appendix B gives a complete list of all the variables included in the first stage. We use the full sample to estimate this specification, and the results are reported in Column IV of Table 5.

The results in Table 5 are consistent across all specifications. As predicted in Empirical Implication 2(ii), the size of the issuance increases with past returns (*PRTN*). The t-statistics 4.3, 3.6, and 2.6 in Columns I, III, and IV, respectively, of Table 5.[16] The bootstrapped results also indicate that this effect is significant at less than the 5 percent level. In other words, companies tend to issue larger amounts of equity when market conditions are good. However, this effect is less important for companies that are audited by large firms (as predicted by Empirical Implication 2(iii)). The interaction of past return with *BigSix* is negative in all specifications. The t-statistics are −2.3, −3.7, and −2.2 in the different specifications.[17] The result also holds in our bootstrapped specification (Column II, Table 5). The effect is also economically significant; being audited by a large firm reduces the sensitivity to past returns by more than half.[18] Finally, we find that companies audited by large firms tend to issue larger amounts of equity when they do issue (consistent with Empirical Implication 1(ii)). The t-statistics are 4.0, 3.7, and 2.1 in Column IV of Table 5. The coefficient is significant at the 5 percent level in our bootstrapped specification. Untabulated results show that when we run our tests separately for different size groups, the interaction effect is statistically stronger for smaller companies.

V. FURTHER ANALYSIS

The previous section established the effect of auditor quality on the issuance of debt and equity. We now explore the causes and consequences of this effect empirically.

The Consequences of Auditor Quality on Capital Structure

Both our theoretical developments and the results in Tables 4 and 5 relate to the effects of market conditions and auditor quality on debt and equity issuance. The recent literature looks at a company's debt ratio to investigate the implications for issuance activity in response to market conditions (see Baker and Wurgler [2002], Leary and Roberts [2005], Kayhan and Titman [2007] for examples of papers using this approach). The essential idea is that if companies do not follow very closely a target capital structure (either because of adjustment costs such as underwriting fees or because they really have no debt ratio targets), then issuance activity will have persistent effects on debt ratios, at least in the medium term. Baker and Wurgler (2002) and Kayhan and Titman (2007) examine whether the sensitivity of past equity issues to market conditions has a negative impact on subsequent debt ratios or changes in debt ratios, while Leary and Roberts (2005) examine whether the effect of the pattern of issuance is more persistent when firms are likely to face higher adjustment costs (related to issue costs of debt and equity).

Our main findings are that companies are likely to make larger size equity issues (and issue equity more often) when market conditions are good and that these effects are likely to be stronger for companies audited by non-Big 6 auditors. Based on the studies we have discussed, it seems natural to investigate whether our main findings affect debt ratios. To do so, we follow a strategy similar to that employed by Kayhan and Titman (2007). We regress the change in the leverage ratio over a five-year period on stock returns over the same period, a Big 6 indicator variable, and the interaction between the two. Specifically, we estimate the following model:

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[16] The λ coefficient, a statistic for the selectivity effect, in our Heckman estimation (Column IV) is −0.36 and is significant at less than the 5 percent level (the 95 percent confidence interval is [−0.39, −0.33]), suggesting that self-selection is indeed a concern.

[17] Our model assumes that the motive behind equity issuance is to finance new investments. Our sample contains 373 (out of 2,939) instances where companies issue equity to repurchase debt. Excluding these observations leads to slightly more significant results (the coefficient associated with the interaction term becomes −0.033 with a t-statistic equal to −2.7).

[18] For example, the ratio of the coefficients is 0.029/0.056 in the OLS specification.

$$\begin{aligned} Lev_{i,t} - Lev_{i,t-5} = {} & c_0 + c_1 ASR_{i,t} + c_2 BigSix_{i,t-5} + c_3 ASR_{i,t} \times BigSix_{i,t-5} \\ & + c_4 DevTgt_{t-5} + \gamma \Delta C_{i,t} + \varepsilon_{i,t} \qquad (6) \end{aligned}$$

where the dependent variable is the change in the leverage ratio (*Lev*) for company i between year $t-5$ and t. *BigSix* is our previously defined dummy variable. *Accumulated Stock Return* (*ASR*) is obtained by compounding monthly stock returns from year $t-5$ to t. *ASR* × *BigSix* represents an interaction between *ASR* and *BigSix*. ΔC represents the change in the control variables over the same period. These control variables are essentially those used in Equations (3) and (4) but we make the following changes. We drop the control variables related to the change in *R&D* and earnings volatility (measured over the entire life of the firm in Compustat) from this test as they are essentially fixed firm characteristics. In addition, we control for the deviation in the debt ratio at $t-5$ from an estimated target (*DevTgt*). If the effect of market conditions has long-lasting implications for a company's capital structure, then we expect that debt ratios will decrease more (as more equity is issued) if stock returns are high during this period (and therefore c_1 should be negative). More importantly, for our purpose, we expect c_2 to be negative and c_3 to be positive if auditor quality has long-lasting effects on a company's capital structure.

We report the results of estimating Equation (6) (with *Accumulated Stock Return*) in Table 6. We use our usual three approaches to estimate the model (OLS in Column I, the two-stage bootstrap procedure in Column II, and the instrumental variable approach in Column III). The results are consistent with our prior specifications. In all cases, the coefficient of *ASR* is negative (t-statistics of -10.0 and -4.3 in Columns I and III, respectively). This is consistent with the idea that adverse selection is time varying and less likely to be important when market conditions are good. However, as expected, the interaction between *BigSix* and *ASR* is positive in all specifications (t-statistics of 4.0 and 3.7 in Columns I and III, respectively). Our bootstrapped specification in Column II generates similar results. These results are consistent with the hypothesis that companies audited by Big 6 firms have less incentive to issue equity when market conditions are favorable. We obtain similar results (untabulated) when we consider changes in a company's capital structure over a ten- (instead of a five-) year period, or if we exclude observations for which *BigSix* changes in value over the five-year period (untabulated results). We also obtain similar results if we use only the years when companies issue securities (debt or equity) to estimate the market leverage ratio. Our results are also similar if we use the book-leverage rather than the market-leverage ratio. The effect is economically significant, and switching to a Big 6 auditor reduces the sensitivity to market conditions by approximately 50 percent.[19] When we partition the sample based on company size, the results (untabulated) indicate that the effect is stronger for the smallest companies but insignificant for the largest ones. Table 6 also indicates that the increase in the debt ratio is less for companies audited by

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[19] The ratio of coefficients is 0.006/0.012 in the OLS specification.

TABLE 6
Auditor Quality, Accumulated Stock Return, and Capital Structure

Change in Leverage Ratio from t−5 to t ($Lev_t - Lev_{t-5}$)	(I) OLS		(II) Two-Stage Bootstrap		(III) Two-Stage Wooldridge	
	Coef.	t-stat.	Coef.	95% Interval	Coef.	t-stat.
Accumulated Stock Return (ASR)	−0.012***	(−10.0)	−0.015	[−0.019, −0.011]	−0.028***	(−4.3)
BigSix	−0.029***	(−6.8)	−0.040	[−0.047, −0.033]	−0.304***	(−12.5)
ASR × BigSix	0.006***	(4.0)	0.005	[0.003, 0.007]	0.026***	(3.7)
ΔTotal Assets	0.064***	(25.7)	0.064	[0.057, 0.071]	0.053***	(20.4)
ΔMarket-to-Book Ratio	−0.006***	(−4.7)	−0.007	[−0.009, −0.004]	−0.006***	(−4.8)
ΔReturn on Assets	−0.110***	(−10.7)	0.011	[−0.076, 0.098]	−0.106***	(−9.6)
ΔTangibility	0.168***	(15.6)	0.172	[0.150, 0.194]	0.165***	(13.7)
ΔUnleveraged Z-Score	−0.002**	(−2.3)	−0.003	[−0.008, 0.002]	−0.002**	(−2.3)
ΔDividend-to-Assets Ratio	0.016	(0.9)	0.011	[−0.026, 0.049]	0.009	(0.5)
ΔStock Return Volatility	1.507***	(17.8)	1.473	[1.285, 1.661]	1.460***	(16.9)
ΔShare Turnover	−9.323***	(−2.8)	−9.346	[−16.049, −2.644]	−8.609***	(−2.8)
Deviation from Target Debt Ratio	−0.328***	(−35.8)	−0.349	[−0.371, −0.328]	−0.350***	(−30.1)
ΔAnalyst Following	−0.002***	(−9.7)	−0.002	[−0.023, −0.017]	−0.002***	(−9.0)
ΔMarginal Tax Rate	−0.112***	(−11.2)	−0.151	[−0.188, −0.114]	−0.087***	(−7.6)
Debt Rating	0.027***	(10.9)	0.055	[0.047, 0.063]	0.055***	(14.6)
R^2/Firm-years	0.33/39,837		0.33/500 replications		0.09/39,837	

Coefficients that are significant at the 10 percent, 5 percent, and 1 percent level are marked with *, **, and ***, respectively.

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The dependent variable is *Change in Leverage Ratio from t−5 to t. Accumulated Stock Return (ASR)* is obtained by compounding monthly stock returns from *t*−5 to *t*. Constant terms and year dummies are included in the regressions, but not reported. The control variables are defined in Appendix B. Column I presents the OLS specification. The t-statistics in parentheses are calculated from the Huber/White/sandwich heteroscedastic consistent errors, which are also corrected for correlation across observations of a given firm. Column II reports the regression using a two-stage procedure. In the first stage, the likelihood of the firm selecting a Big 6 auditor is predicted based on firm-specific variables using the probit model in Table 3. In the second stage, *Change in Leverage Ratio* is regressed on predicted auditor quality, *ASR*, the interaction term between predicted auditor quality and *ASR*, and the changes in other control variables. We bootstrap the system 500 times to obtain consistent standard errors. The 95 percent confidence intervals in square brackets are calculated from 500 bootstrap replications resampled from the data set with replacement of clusters. The average R^2 of 500 replications is reported. Column III reports the results from an instrumental variable approach, which is described in Wooldridge (2002).

Variable Definitions:

BigSix = dummy equal to 1 if a firm chose a Big 6 auditor at *t*–5, and 0 otherwise; and

Deviation from Target Debt Ratio = difference between a firm's leverage ratio at *t*−5 and the target debt ratio estimated in Appendix C.

Big 6 firms. The control variables have signs essentially consistent with those documented by Kayhan and Titman (2007).

As an additional robustness check, similar to Fama and French (2002) and Flannery and Rangan (2006), we estimate a target adjustment model to investigate whether auditor quality also has a direct effect on a company's capital structure by regressing the leverage ratio at time *t* on its lagged value, *BigSix*, and the different control variables described in Appendix B.[20] Untabulated results indicate that *BigSix* significantly reduces the target leverage ratio (t-statistic = −2.7). This is true for our overall sample and across all size groups, and the effect is stronger for small companies.

These additional results are consistent with the idea that auditor quality has a long-lasting effect on the financial structures of companies.

The Causes of the Effect of Auditor Quality on Financial Decisions

Having explored some of the consequences of our main findings in the previous section, we investigate the causes of the different auditor effects that we document in our empirical tests. We consider two possibilities. The first is the insurance role played by auditors, and the second is the reduction in information asymmetry through an improvement of a firm's disclosure policy, and especially its accounting reporting.

The idea behind the insurance effect is that Big 6 firms have deeper pockets than do smaller auditors in cases of litigation and thus offer more valuable insurance in case the initial value of the firm had been inflated. If this insurance effect is the primary driver of our results, then the effect of being audited by a Big 6 firm should be stronger when the expected liability is greater. We interact either an indicator variable for industries with high litigation risk (Hogan 1997) or the Z-score (Dye 1993, which indicates that the greatest liability of auditors derives from auditing clients that subsequently experience financial distress) with *BigSix* in the debt-equity probit regression (described in Equation (3)) and in our capital structure regression (reported in Table 6). The interactions are insignificant, which suggests that the insurance effect does not drive our results.

To investigate the second possibility, we regress the probability of informed trading (*PIN*) (Easley et al. 2002), a measure of accrual quality (Francis et al. 2005), and a measure of discretionary accruals (Teoh et al. 1998a) on *BigSix*, controlling for firm-specific variables used in Equation (3). The results (untabulated) indicate that the probability of informed trading and the amount of discretionary accruals are significantly lower among companies that are audited by a large firm, whereas the accounting quality is greater. This finding holds both in the OLS specification and the specifications designed to address the endogeneity of auditor quality (bootstrapped procedure and 2SLS approach). These additional results are consistent with those of previous studies, including Becker et al. (1998), Teoh and Wong (1993), and Krishnan (2003). These prior results and our new findings collectively support the idea that large auditors reduce information asymmetry through an improvement of the disclosure policy of firms, and especially their accounting reporting.

VI. ROBUSTNESS CHECKS

We now explore different empirical issues to check the robustness of our results.

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[20] More specifically, we start with the following model: $Lev_{i,t} - Lev_{i,t-1} = \alpha_1 + \lambda[Lev^*_{i,t} - Lev_{i,t-1}] + \varepsilon_{i,t}$, where $Lev_{i,t}$ is the leverage ratio of company *i* in year *t*, $Lev_{i,t-1}$ is the leverage ratio lagged one period, and λ is the speed of adjustment. $Lev^*_{i,t}$ denotes the target leverage ratio, which can be expressed as a function of auditor quality and a set of predetermined variables (C): $Lev^*_{i,t} = \alpha_2 + \beta C_{i,t-1} + \delta BigSix_{i,t-1}$. By combining two equations, we have the following reduced form: $Lev_{i,t} = \alpha_3 + (1 - \lambda)Lev_{i,t-1} + \mu BigSix_{i,t-1} + \gamma C_{i,t-1} + \varepsilon_{i,t}$, where $\gamma = \lambda\beta$ and $\mu = \lambda\delta$. Thus, the impact of *BigSix* on the target leverage ratio is then equal to μ/λ.

The Effects of Endogeneity, Causality, and Omitted Variables

Differentiating between correlation and causality is probably the most difficult problem in empirical economics, and it is doubtful that any analysis can fully address it. Nevertheless, we consider several tests that speak to this issue. First, as noted above, we use different econometric techniques to deal with the endogeneity of auditor selection. For example, we employ the instrumental approach described by Wooldridge (2002). Consistent with a directional relation, our results hold when we use these different procedures. In addition, company size is the largest driver of auditor choice. To ensure that our results are not driven by company size, we form a matched sample based on two-digit SIC industry codes and size. For each company audited by a small auditor, we identify a match that is in the same two-digit SIC industry, has total assets as close as possible, and is audited by a Big 6 firm. We then re-estimate our different models using this new sample. Our results (untabulated) are qualitatively similar.[21]

Second, we exploit the fact that auditor quality is sticky. It seems reasonable to argue that if auditor quality has been the same for years, then it is unlikely to be a by-product of current financial choices. Therefore, we rerun all of our tests using only firms with auditor tenures longer than seven years. The degree of endogeneity is likely to be less in this sample. All of our results (untabulated) hold. However, a problem with this approach is that the endogeneity may be driven by time-invariant characteristics.

As a third test, we re-estimate our models using company-level fixed effects to rule out this possibility. The statistical significance of our most important results improves when we include a company-level fixed effect (compared with the cross-sectional OLS results). To obtain more variation in our auditor quality variable, we use a fourth approach and replicate the interaction tests on a subsample of companies that switched auditors. This sample is small compared with our full sample. It contains 2,385 observations and covers 147 small-to-big auditor-switching events and 128 big-to-small auditor-switching events. Not surprisingly, the t-statistics of our interaction variables are reduced compared to our OLS cross-sectional tests (-2.1 for the equivalent in Table 5 and 1.9 for the equivalent in Table 6 versus -2.3 and 4.0, respectively, in our main sample), but the signs are not affected. Our results also hold in the Heckman specification in Table 5 (t-statistic $= -2.0$). These additional results support the argument that there is a causal relation between an increase in auditor quality and lower sensitivity to adverse selection in the issuance decisions made by companies.

The Effect of National Auditors

Our main tests consider the effect of Big 6 firms on financing decisions. We further refine our auditor classification by partitioning the non-Big 6 firms into national auditors (BDO Seidman, Grant Thornton, McGladrey and Pullen, and Richard A. Eisner) and small ones.[22] We replicate our main tests for companies that are not audited by Big 6 firms and substitute an indicator variable that is equal to 1 for companies audited by a national auditor, and 0 otherwise. We have to restrict our sample to the post-1987 period because information on national auditors is not available before that time. The results (untabulated) indicate that national auditors have a significant impact on financing decisions: the variable is significant in the debt/equity probit model (t-statistic = −2.0). In our tests of the effect of market conditions on capital structure, the interaction between the new dummy and past stock returns is significantly positive (t-statistic = 1.9). However, the new dummy is insignificant in our size of equity issuance (*Esize*) regression, perhaps because of the significantly reduced sample size and much smaller number of equity issues. Taken together, these results suggest that compared with smaller auditors, national auditors also mitigate information asymmetry, but not as well as Big 6 firms do. However, this weaker significance has to be qualified because the drastic reduction in the sample size reduces the power of our tests.

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[21] The one exception is in the matched sample for Equation (6). The coefficient of the interaction term between *ASR* and *BigSix* becomes insignificant but remains positive in the three-step procedure described by Wooldridge (2002). The results of the OLS and bootstrapped procedures hold.

[22] Specifically, a firm is considered to be audited by a national auditor if the audit code (Compustat item 149) equals 11, 17, 21, or 26.

VII. CONCLUSION

We find evidence that the difference in information asymmetry associated with high-quality auditors affects the financing choices of companies. First, companies audited by large auditors are more likely to issue equity as opposed to debt and to have more equity in their capital structures. Second, companies tend to issue larger amounts of equity when market conditions are favorable. This association is significantly weaker for companies with Big 6 auditors. Third, the debt ratios of companies audited by Big 6 firms are less affected by market conditions. These results hold in different specifications that are designed to minimize the risk of bias due to the endogeneity between auditor quality and financing decisions. They also hold both cross-sectionally and in panel settings, and in a sample of companies that switch between small and large auditors.

APPENDIX A

PROOF OF PROPOSITION 1 PART (iii)

Fix I, and suppose the equilibrium is one in which type $X = X_H$ with a poor-quality auditor issues equity. If, instead, it issues debt, and assuming without loss of generality that the debt holders' required rate of return is zero, then the face value of the debt is given by $F = I/\delta$, and hence the payoff to the company is:

$$\Pi_D = \delta(I + V + X_H - F) - (1 - \delta)c = \delta(X_H + V) + (\delta - 1)I - (1 - \delta)c. \tag{A1}$$

Hence:

$$\frac{d\Pi_D}{dI} = \delta - 1 < 0. \tag{A2}$$

Suppose instead that the company issues equity. Let α be the fraction of equity offered to outside investors. Then we have:

$$\alpha\delta[\text{Prob.}(X = X_H|X_H \text{ is reported})X_H + \text{Prob.}(X = X_L|X_H \text{ is reported})X_L + I + V] \\ = I. \tag{A3}$$

Denote:

$$E(X, p) = \text{Prob.}(X = X_H|X_H \text{ is reported})X_H + \text{Prob.}(X = X_L|X_H \text{ is reported})X_L \\ = \frac{2p}{1 + p}X_H + \frac{1 - p}{1 + p}X_L.$$

where the last equality follows from Bayes' Rule.

The payoff to the company's existing equity holders is:[23]

$$\Pi_E = (1 - \alpha)\delta(X_H + I + V) = \delta(X_H + I + V) - \frac{I}{E(X, p) + I + V}(X_H + I + V). \quad \text{(A4)}$$

It is easily checked that $E(X, p)$ is increasing in p. Also:

$$\frac{d\Pi_E}{dI} = \left(\delta - \frac{X_H + I + V}{E(x, p) + I + V}\right) + \frac{I(X_H - E(x, p))}{(E(X, p) + I + V)^2}$$
$$= (\delta - 1) + \frac{X_H - E(X, p)}{E(X, p) + I + V}\left(\frac{I}{E(X, p) + I + V} - 1\right) < \delta - 1 < 0. \quad \text{(A5)}$$

Notice that, for a given p and sufficiently small I—in particular, I satisfying condition (2)—the expression in (A4) exceeds that in (A1). Hence, the company will issue equity, and the proposed beliefs that the market has are supported.

However, by virtue of (A5) and (A2), for a sufficiently high I, the expression in (A4) will be less than that in (A1). Hence, the company will issue debt, and the belief must be that a company issuing debt is type X_H. Notice, however, that type X_L with a low-quality auditor, (whether it succeeds in misreporting), has no incentive to mimic this type by issuing debt. At worst, the equity is correctly priced for such a type, and while there is no mispricing gain from debt issuance, there is a potential financial distress cost. Therefore, if a company with a low-quality auditor issues equity, then the market must believe that the true earnings are $X = X_L$. It is easy to check that these beliefs support the equilibrium issuance decision of type $X = X_H$.

Finally, notice that for any I and for p sufficiently close to 1, the expression in (A4) exceeds that in (A1) (and is positive as the project is assumed to have a positive net present value). Hence, a set of beliefs for which type X_H with a poor-quality auditor issues equity for both I_1 and I_2 can be supported.

APPENDIX B
VARIABLE DEFINITIONS

Variable	**Definition**
Esize	Net equity issue equals the sale of common and preferred stock minus the purchase of common and preferred stock. Net debt issue equals long-term debt issuance minus long-term debt reduction plus changes in current debt. *Esize* is equal to the amount of net equity issue deflated by the book value of assets at the beginning of the fiscal year.
BigSix	Dummy variable equal to 1 if the audit code (Compustat item 149) is between 1 and 8, and 0 otherwise.
Leverage Ratio (*Lev*)	Total debt/(total assets + market value of equity − book value of equity).

(*continued on next page*)

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[23] We assume that V is sufficiently large so that the expression below is positive at $I = I_1$.

Size of Total Financing	Total financing needs/total assets = (net debt issued + net equity issued)/total assets.
Log (Assets)	Log of total assets.
Age	Number of years since the firm entered Compustat.
Tangibility	Net PPE-to-assets ratio.
Return on Assets	Ratio of operating income before depreciation and amortization to total assets.
Share Turnover	Median value of monthly shares traded (volume) divided by shares outstanding over a 12-month period.
Past Stock Return	Compounded annual stock return over a 12-month period.
Market-to-Book Ratio	(Total assets + market value of equity − book value of equity)/total assets.
R&D to Sales Ratio	Research and development expenses scaled by total sales.
R&D Dummy	Dummy variable equal to 1 if research and development expenses are missing, and 0 otherwise.
Dividend-to-Assets Ratio	Ratio of dividends to total assets.
Unleveraged Z-Score	(3.3 × pretax income + sales + 1.4 × retained earnings + 1.2 × [current assets − current liabilities])/total assets.
Stock Return Volatility	Standard deviation of the daily stock return calculated for each firm for each year.
Earning Volatility	Historical standard deviation (using available data during the previous ten years) of the ratio of EBITDA to total assets.
Marginal Tax Rate	Simulated marginal tax rate after interest expenses obtained from John Graham's website.
Debt Rating	Dummy variable equal to 1 if the firm has a debt rating assigned by Standard & Poor's, and 0 otherwise.
Analyst Following	The maximum number of analysts making annual earnings forecasts any month over a 12-month period. We assume that companies not covered by I/B/E/S have no analyst coverage.

Variables in the selection equation of Table 5, Column IV

(Apart from the variables listed below, we also include in the selection equation *Unleveraged Z-Score*, *R&D Expenses*, *Market-to-Book Ratio*, *Tangibility*, *Marginal Tax Rate*, *Analyst Following*, and *Debt Rating Dummy*, which are defined above.)

Projected Current Investment	Average investment growth rate over the previous three years multiplied by the previous year's investment.
Cash Balance	Sum of cash and marketable securities divided by total assets.
Projected Current Cash Flow	Average cash flow growth rate over the previous three years multiplied by the previous year's cash flow.
Industry Equity Issue	Proportion of firms issuing equity in the industry (two-digit SIC code).
Anticipated Future Investment	Actual investment to total assets ratio over the next year.
Anticipated Future Cash Flow	Actual cash flow to total assets ratio over the next year.
Dividend Payer Dummy	Dummy variable equal to 1 if a company paid dividends, and 0 otherwise.
New Working Capital	(Other current assets − [total current liabilities − short term debt])/total assets

APPENDIX C
ESTIMATION OF THE TARGET LEVERAGE RATIOS

The sample includes all Compustat industrial companies with complete data for five or more consecutive years from 1985–2005. The dependent variable, *Leverage Ratio* (*Lev*), is total debt divided by (total assets − book equity + market capitalization). The control variables are defined in Appendix B and lagged one period relative to the dependent variable (*Lev*). The median industry leverage ratio is defined as the median of the ratio of total debt to the market value of assets by the three-digit SIC code and by year. Coefficients that are significant at the 10 percent, 5 percent, and 1 percent level are marked with *, **, and ***, respectively. The constant terms are included in the regression but not reported. The t-statistics in parentheses are calculated from the Huber/White/sandwich heteroscedastic consistent errors, which are also corrected for correlation across observations for a given firm.

	OLS	
***Leverage Ratio* (*Lev*)**	**Coef.**	**t-stat.**
Log (*Assets*)	0.009***	(5.6)
Log (*Age*)	−0.016***	(−5.0)
Tangibility	0.069***	(7.1)
Industry Median Leverage Ratio	0.438***	(24.5)
Return on Assets	−0.025	(−1.6)
Share Turnover	−7.515***	(−6.3)
Past Stock Return	−0.014***	(−13.3)
Market-to-Book Ratio	−0.017***	(−11.1)
R&D-to-Sales Ratio	−0.018***	(−6.6)
R&D Dummy	0.021***	(5.6)
Unleveraged Z-Score	−0.010***	(−7.3)
Dividend-to-Assets Ratio	−0.181***	(−2.9)
Stock Return Volatility	1.306***	(11.1)
Earning Volatility	−0.001***	(−6.4)
Marginal Tax Rate	0.095***	(7.0)
Analyst Following	−0.003***	(−14.4)
Debt Rating	0.072***	(16.3)
R^2/Firm-years	0.30/39,837	

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Promotions, Turnover, and Performance Evaluation: Evidence from the Careers of Division Managers

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ABSTRACT: We study turnover and promotions of division managers in multidivisional firms. Turnover is negatively related to divisional accounting performance, positively related to industry performance, but not significantly related to firm performance or the performance of other divisions. Consistent with tournament theory, promotions are significantly related to whether one division is performing better than others, but are not significantly related to the magnitude of any performance difference. A simple performance metric, divisional ROA, appears more closely related to job allocation decisions than several alternatives. Our evidence is consistent with the hypothesis that accounting information is used by firms when evaluating managerial personnel.

Keywords: *turnover; promotions; internal labor markets; performance measurement; division managers.*

Data Availability: *All data are derived from public sources identified in the paper.*

JEL Classifications: *J41; G34; M51; L22.*

I. INTRODUCTION

How is managerial performance measured and rewarded in organizations? While much has been learned concerning chief executive officers (CEOs), comparatively little is known about other senior executives. Since collectively these managers

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should have a substantial effect on firm value, understanding how they are evaluated is an important question. We examine this issue by studying job changes of division managers in multiple-division firms. In particular, we study penalty outcomes in which managers depart from their employers, and reward outcomes in which managers are promoted from within the firm. If individual accounting performance metrics provide useful information regarding effort or ability, then we expect to observe a relation between these metrics and job changes.

In our analysis, we address two distinct but related issues. First, we consider the issue of *what* performance metrics are used in evaluating managerial personnel. Since non-CEO dismissals are significantly less related to stock returns than they are for CEOs, there may be an important role for the firm's accounting system to create useful information regarding individual performance. The work of Holmström (1979) predicts that firms should rely on the accounting metrics that are most informative with regard to managerial ability or effort, but identifying these metrics is necessarily an empirical issue. The second issue we address is *how* various performance measures are used. Following Holmström (1982a), the theory of relative performance evaluation (RPE) predicts that an individual's performance should be benchmarked against the performance of others in the same industry or the same firm. However, the benefits of this benchmarking may be offset by certain costs—for example, excessive incentives to sabotage the competition (e.g., Lazear and Rosen 1992; Aggarwal and Samwick 1999). In our analysis we assess the extent and form of performance benchmarking behavior.

We find that there is a strong negative relation between division manager turnover and divisional return on assets (ROA). This evidence strongly supports the hypothesis that divisional accounting information provides information that is useful in deciding whether to retain a manager. Moreover, our results suggest that a simple metric, ROA, is relied on in making these decisions. We also find evidence indicating that firms conduct RPE by filtering out industry performance when choosing to dismiss managers. However, we find little convincing evidence that division managers are benchmarked against their internal same-firm counterparts in turnover decisions. These findings on division manager turnover appear robust to the inclusion of numerous control variables and a variety of different ways of modeling the turnover process.

Turning to promotions, we find that managers at divisions with higher levels of ROA than their counterparts within the firm are significantly more likely to be promoted. Moreover, we find that the sign of the performance difference between divisions is a more important predictor of promotions than the magnitude of any such difference. This suggests that division managers will care a great deal about winning the race against their counterparts at other divisions, but little about the margin of victory. More generally, our evidence is consistent with a fundamental assertion of tournament theory, namely, that managers will be promoted based on ordinal rankings of performance (e.g., Lazear and Rosen 1981; Rosen 1986).

Our findings suggest that division managers will often have substantial incentives to increase divisional accounting performance. Certainly this will, at times, have a beneficial impact in motivating managers. However, it could also have some deleterious consequences in settings in which the objectives of increasing accounting performance and value maximization diverge. Our evidence also suggests that turnover and promotion decisions do not generate strong incentives for interdivisional cooperation. Superior performance at other divisions does not help a given manager keep her job, and it will lower the likelihood of promotion if it pushes the other divisions ahead of hers.

The analysis we present does have some limitations. Practical considerations dictate that we study a set of firms with stable and well-defined organizational structures. In addition, for much of our analysis, we restrict attention to a fixed set of firms with two divisions that are similar in size. Given these sampling choices, our inferences are most likely to apply more generally to firms with similar characteristics to those that we study. In the case of turnover, we use within-sample variation to show that our results are not entirely driven by the most stable and durable firms in the sample, thus increasing our confidence in the generality of these findings. In the case of promotions, we analyze an auxiliary sample and demonstrate that our findings are robust to an alternative sample selection procedure.

In Section II we discuss the related literature and our empirical strategy. In Section III we outline our sample construction and present descriptive statistics. Our analysis of turnover is reported in Section IV, and in Section V we present the results on promotions. Section VI concludes.

II. RELATED LITERATURE AND EMPIRICAL DESIGN

Compensation and Turnover

A small literature examines the compensation-based incentives of executives below the CEO (e.g., Murphy 1985; Leonard 1990; Fisher and Govindarajan 1992; Bushman et al. 1995; Keating 1997; Aggarwal and Samwick 2003; Barron and Waddell 2003; Wulf 2007). These papers report that senior executives are compensated based on both firm performance and individual performance, with more weight placed on individual performance when it is a more precise signal of managerial effort or ability. In the case of division managers, Aggarwal and Samwick (2003) find that firms rely on divisional accounting figures in setting division manager compensation.

In addition to compensation-based incentives, the possibility of removal from office following poor performance may serve as a powerful incentive device for corporate executives. Job dismissals can be viewed as a component of an incentive system to motivate effort from managers. Alternatively, turnover can be viewed as part of an ability screening mechanism intended to optimally assign individuals to managerial positions. Under either view, turnover should be related to measures of individual performance.

Given the potential importance of turnover policy as a determinant of incentives and firm performance, a small set of studies have examined this issue for non-CEO executives. Fee and Hadlock (2004) find that non-CEO dismissals are less related to firm performance than are CEO dismissals (see also Hayes et al. 2006). While this evidence suggests that firm performance is a noisier signal of effort or ability as one descends through the hierarchy, it does not identify the alternative metrics that may be relied upon in evaluating senior executives. In the special case of banks, Blackwell et al. (1994) report that firms often turn to accounting information in the evaluation process. In particular, they report that turnover of bank subsidiary managers is negatively related to a unit's own ROA and positively related to the ROA of the bank's other subsidiaries. Finally, when compared to CEOs of focused firms, McNeil et al. (2004) report that division manager turnover in conglomerates is relatively more sensitive to a unit's accounting performance.

These prior studies uncover some interesting and important findings regarding turnover. However, we are able to add substantially to this prior evidence by offering the first comprehensive analysis of the relation between different accounting performance metrics and division manager turnover for firms spanning a broad set of industries. As we interpret our turnover results, we discuss how they compare to some of these earlier studies.

Internal Promotions

Promotions are important events both for executives and for their employers (see Baker et al. 1988). However, despite the importance of these events, our understanding of the determinants of promotion decisions for senior executives is limited. Several researchers have studied the career profiles of workers within firms (e.g., Rosenbaum 1979; Medoff and Abraham 1980; Sheridan et al. 1990; Lazear and Rosen 1992; Baker et al. 1994a, 1994b; Gibbs 1995).[1] While this literature has generated many interesting findings, it does not offer direct evidence on the link between accounting measures of performance and promotions near the top of the corporate hierarchy.

In one of the few studies that examine promotion decisions for senior executives, Blackwell et al. (1994) find that number two executives at bank subsidiaries are more likely to be promoted within the subsidiary when the unit is performing well. While this is a useful finding for understanding the decision to promote from within, it does not identify the role of individual performance in choosing among a set of candidates. Progress on this issue has been restricted to certain specialized labor markets. In particular, Kwon (2006) reports that promotions of insurance claim processors are related to individual productivity measures, and Fee et al. (2006) report that assistant football coach promotions are positively related to individual performance metrics.

As is illustrated in the seminal model of career concerns by Holmström (1982b), many theories of job allocation decisions predict that a firm's assessment of ability will be a primary determinant of promotions. If this is the case, and if divisional accounting figures provide information on ability, then we should observe a positive relation between accounting performance and promotion likelihood. Our analysis adds to prior studies of promotions by directly testing for the first time this important hypothesis for executives near the top of the corporate hierarchy.

As has been emphasized by prior authors, promotion opportunities within a firm may be limited. Thus, an executive's performance relative to others may be the relevant measure that determines the likelihood of a promotion. If firms rely heavily (or exclusively) on individual measures of performance in promotions, then small performance differences between managers could result in large differences in promotion probabilities. Taking this logic one step further, tournament theory models (e.g., Lazear and Rosen 1981) predict that firms may deliberately emphasize small differences in performance when making promotion decisions as a mechanism to elicit high effort.

It is possible, however, that firms rely on many factors not related to performance in making promotion decisions, such as a manager's seniority, the relationship between the manager's division and corporate headquarters, and the specific skill set of an executive. If this is the case, or if accounting performance is only weakly related to ability, then we may not observe any relation between promotions and divisional accounting performance. If we did observe such a relation, then we would not expect very small differences in performance to be associated with large differences in promotion probabilities, since in these cases other factors would be the primary determinants of the promotion decision. Thus, a finding that firms heavily emphasize performance rankings would suggest that accounting performance is a particularly reliable signal of ability and/or that firms deliberately emphasize ordinal rankings for incentive reasons.

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[1] Hayes and Schaefer (1999) and Fee and Hadlock (2003) examine the market for external promotions.

Performance Measurement

The goal of our study is to understand the relation between different measures of performance and division manager job changes (i.e., turnover and promotions). Following the logic of Holmström (1979), we expect a selected indicator of performance to be related to job changes if it is informative in the sense of providing marginal information content regarding managerial effort or ability.[2] As the work of Holmström (1982a) emphasizes, in addition to individual performance, it is possible that the performance of other divisions or other firms in the same industry are informative. Thus, in our analysis, we will examine whether relative performance evaluation can be detected.

While the issue of performance measurement is an important question, our knowledge of this issue for executives below the CEO level is limited because individual measures of performance are typically not available to researchers. However, in the special case of division managers, publicly available segment accounting information can be exploited to construct individual performance metrics. Thus, we select the division manager position for conducting our investigation. We suspect that similar results will apply to other executive positions for which individual performance metrics are available to the firm, even if these measures are not in the public domain.

A variety of accounting metrics may plausibly be related to the evaluation of managerial talent. For example, firms may use a rate-of-return measure such as ROA in evaluating managers. Alternatively, firms may rely on measures of total profitability such as operating profits or net income. Employers may choose to focus on the level of these performance metrics, or alternatively they may look more closely at recent changes. In addition, firms may or may not choose to adjust these measures to filter out market or industry-wide movements. Since theoretical guidance is not strong here, sorting out what metrics are used and how they are used becomes an empirical issue. The initial approach we take below is to follow the treatment of Blackwell et al. (1994) and use a division's level of ROA as our measure of division performance. We then proceed to examine the role of alternative performance metrics.

Our empirical models allow us to assess which performance variables are most closely associated with turnover and promotion outcomes. It is possible that the associations we uncover reflect a direct causal relation between the selected performance metric(s) and job changes. Alternatively, firms may rely on less than perfectly observable metrics that are correlated with the ones that we study. For example, firms may make turnover decisions based on an adjusted version of divisional ROA using adjustments that purge this metric of (unobservable to us) good luck or bad luck events. In this case, if we detect a relation between divisional ROA and turnover, the actual underlying causal relation between turnover and performance would be somewhat different than what our estimates suggest. While this point is subtle, it is worth keeping in mind when interpreting our evidence. As is the case in any study of this type, we are restricted to examining the role of different accounting metrics in capturing or reflecting the underlying performance measures that are used in evaluating managers.

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[2] By marginal information, we mean information that is not completely captured by other variables included in a given empirical model (i.e., the other variables are not sufficient statistics for this information). The notion of contracting on informative signals is equivalent to the idea of contracting on conditionally controllable information (see Antle and Demski 1988). Banker and Datar (1989) consider how much relative weight firms should place on multiple performance metrics.

Limitations of Empirical Analysis

Matching managers with publicly available measures of performance dictates that we study division managers of publicly traded firms. As we describe below, the process of matching managers to divisions is likely to result in a sample of firms that are durable and have stable management and divisional structures. Thus, our results are likely to generalize to other firms with similar features. To gauge how our inferences might apply to other populations, we examine how our findings vary by measures of the stability of a firm's divisional structure. In addition, to consider the specific role of survival, in parts of our analysis we examine differences between earlier and later subperiods.

In our main sample below, we study firms with two divisions and require the divisions to be similar in size. These sampling choices should maximize our ability to detect whether managers are evaluated based on performance measured relative to internal competition, since internal counterparts in these firms can be easily identified. As we report below, in the case of turnover, we find little evidence that managers are evaluated based on performance measured relative to their internal counterparts. Thus, it appears unlikely that we would detect the presence of internal benchmarking behavior for turnover decisions in other more general settings.

In the case of promotions, however, we do find strong evidence that managers are evaluated relative to their internal counterparts. This is an interesting finding, but it leaves unanswered whether a similar result would apply more generally. To address this issue, we augment our main analysis with an auxiliary sample that includes a wider variety of firms. As we report below, the basic character of our results on promotions and performance measurement continues to hold for the auxiliary sample.

III. SAMPLE SELECTION AND DATA DESCRIPTION

Choosing the Initial Sample

Matching managers, divisions, and accounting data requires an intensive investigation into a firm's organizational structure. Thus, we must limit attention to a manageable number of firms. Given this limitation, we choose selection rules that we expect will maximize our ability to test the hypotheses outlined above. We begin with the universe of domestic publicly traded firms listed on Compustat as of the end of fiscal year 1992 and identify all firms with two segments as of this date. From this set, we select all firms for which the sales of each segment are greater than 25 percent and less than 75 percent of total firm sales. By imposing the requirement that segments not be too different in size, we expect to maximize the likelihood that their managers are compared to one another.

We next turn to matching segment accounting information with the division manager responsible for the segment. As is well known, disclosure rules allow firms considerable discretion in defining a segment, particularly prior to the implementation of SFAS No. 131 in 1998. Consequently, in many cases segments do not correspond closely to an actual operating unit with a well-defined management team. Thus, we carefully inspect each firm's listing in the *Directory of Corporate Affiliations* and supplement this with information reported in *Standard & Poor's Register of Corporations, Directors and Executives* and SEC disclosure filings.[3] Using these information sources, we match Compustat segments with units listed in the directories as of the start of fiscal 1993 by carefully comparing names.

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[3] The *Directory of Corporate Affiliations* is a detailed directory of the organizational and management structure of public and private firms used widely by marketing and financial professionals. The *Register of Corporations, Directors, and Executives* is a detailed listing of senior managers and is used widely by human resource professionals.

In many cases this matching is straightforward. When there is not an obvious match, we search through the firm's 10-K statement to further understand its labeling of segments and divisions. Using this information, in many cases we can match a segment to one or more units listed in the directories.[4] If we cannot unambiguously make a match, then we exclude the firm from the sample.

After matching segments with divisions, we assign the division manager title as of the start of fiscal 1993 to the individual listed as the division head in the *Directory of Corporate Affiliations*. In some cases, described further below, we identify multiple individuals heading a division and assign each of them the division head title. We retain in the final sample all firms with two divisions for which at least one manager for each division could be identified as of the start of 1993.

Following the Sample

Using our data sources, we record the identity of the manager(s) of each division up to the end of fiscal year 2000. In some cases, firms increase their number of divisions, often by making acquisitions. In other cases, firms change their focus and start operating as single-division firms, often by divesting assets. In both of these cases we stop following the firm in the first year of the change in corporate structure. There are some cases, however, where a firm starts reporting data for three or more segments, it is evident that the new segment information simply reflects a finer partition of the firm's previous segment definitions. In these cases, we aggregate the information on the newly defined segments so that our definition of what constitutes a division at a given sample firm is comparable over time. The end result is that we construct a sample in which: (1) each firm has exactly two divisions each year, (2) divisions typically consist of a single segment, and (3) in some (post-1993) cases a division comprises more than one segment.

There are cases in which we cannot identify a single division manager. This occurs when the directories list more than one manager as the head of a division and when they list only the heads of subunits of a division. Whenever we cannot identify a single division head, we assign the division manager title to the set of individuals who are associated with the top management of the division. This procedure should match individuals to accounting figures that pertain to, at the very least, a substantial part of the activities they oversee.

The final sample consists of all division-manager-years from 1993 to 2000 for which we can assign at least one manager to each of the firm's two divisions as of the start of the year and for which we can confirm that the firm has two divisions at the end of the year. Summary statistics for the resulting sample of 1,294 observations are reported in Panel A of Table 1. Since firms drop out of the sample over time, the number of observations declines from 268 observations in 1993 to 63 observations in 2000. The mean number of segments in a division is 1.04, illustrating that in most cases a division is composed of a single segment. The mean number of managers at each division is 1.18, indicating that most divisions have a single division manager.

Identifying Job Changes

For each manager in office at the start of a year, we use the directories to determine whether the individual is still serving as division manager at the end of the year. If so, then we refer to this as a "no change" observation. In all other cases, we conduct a news

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[4] In most cases, the Compustat segment corresponds to a single unit in the directories. However, in a few cases we match a segment to a set of subunits listed in the directories and assign the division manager title to the set of individuals heading the subunits. Additional details on these cases are reported below.

TABLE 1
Description of Sample

Panel A: Description of Sample

	Statistic
Number of Firms	122
Number of Firm-Years	548
Number of Division-Years	1096
Number of Manager-Years	1294
Number of Managers in 1993	268
Number of Managers in 2000	63
Mean Number of Segments Per Division	1.04
Mean Number of Managers Per Division	1.18
Mean (larger division sales/smaller division sales)	1.54
Median (larger division sales/smaller division sales)	1.20

Panel B: Turnover Statistics

	# of Obs.	% of Total	Mean ROA	Median ROA
Total Manager-Years	1294	100.00%	14.22%	12.97%
No Change	1113	86.01%	14.32%	12.96%
Depart—Jump	12	0.93%	10.94%	10.56%
Depart—Exogenous	4	0.31%	6.45%	16.55%
Depart—Forced/Demote	13	1.00%	12.73%	10.89%
Depart—Generic	99	7.65%	12.08%	10.53%
Stay—Demotion	1	0.08%	10.26%	10.26%
Stay—Ceremonial	2	0.15%	14.82%	14.82%
Stay—Lateral	15	1.16%	17.33%	17.74%
Stay—Probable Promotion	10	0.77%	22.41%	19.05%
Stay—Definite Promotion	25	1.93%	16.48%	14.78%

The sample includes all manager-years for a sample of firms with two divisions from 1993 to 2000. Divisional accounting data is derived from the segment data listed in the Compustat database. Number of segments and managers per division are calculated by treating each division-year as one observation, while relative division size is calculated treating each firm-year as one observation. In Panel B we report statistics on a manager's fate as of the end of each observation year and all observations are assigned to a single category. A detailed description of each job change category is reported in the text. Divisional ROA is defined as divisional operating profit after depreciation in the fiscal year prior to the observation year divided by (end of year) total divisional assets. The mean and median ROA figures are calculated over the set of observations (i.e., manager years) in the indicated row.

search in the Factiva database. In many cases we can confirm that there was no substantive change in the individual's job title or responsibilities. Presumably these are cases in which the directories are not consistent in assigning the division head title. We treat these cases as "no change" observations. As we report in Panel B of Table 1, 86.01 percent of all observations fall into the no change category.

All cases of substantive job changes are assigned to one of several mutually exclusive categories. The first four categories relate to actual departures from the employer. As we report in Table 1, 128 observations are placed into one of these four categories, implying a departure rate of 9.89 percent. The "jump" category includes job changes in which an individual leaves the firm and immediately accepts employment elsewhere, the "exogenous" category includes job changes related to exogenous events such as poor health, death, or a corporate control event, the "forced/demote" category includes cases in which news articles indicate that the individual was forced from office or the individual was demoted and leaves soon thereafter. All remaining departure events are placed in the "generic" turnover category. Since the evidence in Fee and Hadlock (2004) suggests that most generic departures represent involuntary job changes, in our subsequent analysis we group forced departures and generic turnover together. We include age controls to allow for the possibility that turnover for some older managers may reflect voluntary normal retirements rather than involuntary forced departures.

The other job change categories represent instances in which an individual stays with the firm but experiences a substantive change in job title. As a group, these cases account for 53 of the 1,294 sample observations (4.10 percent). As we report in Table 1, there is only a single instance in which a division manager stays with the firm after an apparent demotion and two cases in which managers are reassigned within the firm to what appear to be ceremonial positions (vice-chairman with no executive responsibilities, head of special projects). The very low rate of demotions we detect is consistent with the findings of labor economists who report a similar result for non-executive employees (e.g., Baker et al. 1994a). This suggests that infrequent demotions are a general feature of employment contracting.

For the remaining cases, we try to ascertain whether the internal job change represents a promotion. If the individual's new job title is corporate CEO, president, COO, CFO, or chief administrative officer, we assign the observation to the "definite promotion" category. This is the most frequent type of internal job change, representing 1.93 percent (25 of 1,294) of all sample observations. For the remaining cases, if the individual (1) assumes a position that has some general corporate oversight or (2) keeps their old position but is given additional new responsibilities, we assign the observation to the "probable promotion" category. This category represents 0.77 percent of all observations. All other cases, representing 1.16 percent of the sample, are placed in the "lateral" category. Since demotions are rare, we suspect that many of these cases actually represent promotions.

Firm, Division, and Manager Characteristics

As we discussed earlier, our initial measure of performance is a division's ROA.[5] Panel B of Table 1 reports that job changes in which an individual departs from the firm tend to be associated with performance falling below the sample mean, while the opposite is true for the lateral move and promotion categories. Formal tests of the difference in ROA between individuals who are promoted (grouping probable and definite promotions together) and those who are potentially dismissed (grouping forced departures and generic departures together) are significant at the 1 percent level for both means (using a t-test) and medians (using a Wilcoxon rank-sum test). This initial univariate evidence suggests a link between performance and job outcomes.

Summary statistics for other firm, division, and manager characteristics are reported in Table 2. Most of these variables are derived from Compustat data, but manager age is collected from news searches and corporate filings and is thus missing more frequently. The typical division manager is about 50 years old, indicating that many of these individuals

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[5] All performance variables (accounting-based and market-based) are winsorized at the 1st and 99th percentile levels. ROA is defined as operating profit divided by end-of-year book assets.

TABLE 2
Sample Summary Statistics

	Sample Mean	Sample Median	Compustat Mean	Compustat Median
Firm Size (Assets)	1,631.13	456.56	2,597.53	181.28
Firm Size (Sales)	1,479.25	474.86	1,195.82	123.32
Division Size (Assets)	710.76	179.49		
Division Size (Sales)	727.91	217.02		
Manager Age	50.92	50.00	53.65	53
Division ROA	14.22%	12.97%		
One-Year Change in Division ROA	0.37%	0.58%		
Three-Year Change in Division ROA	0.84%	1.00%		
Counterpart ROA	15.04%	13.98%		
Industry ROA	8.11%	7.97%		
Firm ROA	9.73%	9.19%	7.92%	5.76%
One-Year Excess Stock Return	1.32%	−0.78%	0.46%	−0.14%

All firm, division, and manager characteristics are measured as of the start of the fiscal year. Asset and sales figures are inflation adjusted to millions of 1999 dollars. A division's one-year (three-year) change in ROA is defined as the division's ROA in the annual period ending at the start of the observation year less the division's ROA in the annual period ending one year prior to (three years prior to) the observation year. For each division-manager-year, Counterpart ROA is the ROA of the firm's division not led by the manager. Industry ROA is the median ROA of all segments that operate in the same two-digit SIC industry as the division. If a division is composed of more than one segment, we use the simple mean of each segment's industry ROA to calculate the division's industry ROA. Firm ROA is defined as the firm's operating income after depreciation divided by end-of-year total assets. The one-year Excess Stock Return is calculated as the firm's stock return over the fiscal year ending immediately prior to the observation year less the CRSP value-weighted index return. The figures in the first two (latter two) columns are for our sample of division managers (all Compustat firms from 1993 to 2000). Manager age reflects the age of the division manager or the CEO.

are sufficiently far from retirement that career concerns could have a substantial impact on incentives. The median firm in our sample is slightly larger than the median Compustat firm, while the median division manager in our sample is slightly younger than the median CEO of a Compustat firm. Additionally, our sample firms appear to be more profitable (as measured by ROA) than the median Compustat firm.

IV. DIVISION MANAGER TURNOVER

Baseline Models

In this subsection we estimate simple logit models predicting turnover. If better performance increases a firm's assessment of a manager's effort or ability, we expect to observe a negative coefficient on performance in these models. In our initial models we treat each division-manager-year as a single independent observation, but later we relax this assumption. The dependent variable is assigned a value of 1 for forced and generic departures, 0 if the manager experiences no job change, and missing for all other job changes. Since turnover rates are often elevated when managers approach retirement age (e.g., Coughlan and Schmidt 1985; Weisbach 1988), we exclude managers aged 63 or over from the estimated models.[6]

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[6] The character of our main turnover and promotion results is robust to eliminating age restrictions and also to including only the oldest division manager under the age of 63 when a division has multiple managers.

In column 1 of Table 3, we report results for a model in which divisional ROA is the only performance measure. The estimated coefficient on this variable is negative and significant (t = −2.14), indicating that poor performance is associated with an elevated likelihood of departure. The economic magnitude of the relation appears substantial, as the implied probability of division manager turnover when the division is at the 90th percentile performance level (ROA of 29.33 percent) is 6.11 percent, increasing to 10.22 percent when the division's performance is at the 10th percentile level (ROA of 0.84 percent).[7] This evidence supports the hypothesis that accounting metrics are used in evaluating managerial personnel.

In column 2 of Table 3 we consider the issue of industry benchmarking of performance. We include in this model industry ROA, defined as the median ROA of all segments that operate in a division's two-digit SIC industry. If firms filter out the industry component of performance when evaluating managers, then we would expect to observe a positive coefficient on this variable, as better overall industry performance indicates poorer relative performance. The estimated coefficient on industry performance in this model is positive and significant, while the coefficient on divisional performance remains negative and significant. These estimates are consistent with the hypothesis that firms consider industry performance when making dismissal decisions. In column 3 we replace the division and industry performance variables with a single measure of industry-adjusted performance defined as divisional performance less industry performance. As expected, the coefficient on this variable is negative and highly significant, indicating that poorer relative-to-industry performance is associated with higher turnover.[8]

Next we consider the role of performance in other parts of the firm on turnover by adding to the logit models the performance of the firm's other division (referred to hereafter as the counterpart division). In column 4 (column 5) of Table 3 we include the raw ROA (industry-adjusted ROA) of both the division and the counterpart division. As we report, in both columns the coefficient on counterpart performance is small in magnitude and insignificant. Thus, in contrast to Blackwell et al. (1994), our sample offers little evidence of within-firm comparisons of divisional managers when making turnover decisions. We discuss possible reasons for this difference in more detail below.

In column 6 of Table 3 we include a firm's ROA as an additional explanatory variable. The coefficient on this variable is small, negative, and insignificant. The significance level of the coefficient on divisional performance drops slightly in this specification (to the 10 percent level), which is not surprising given the correlation between division and firm performance. In column 7 we instead use a firm's one-year excess stock return as a measure of firm performance. The coefficient on this variable is also negative and insignificant, while the coefficient on division performance is significant at the 5 percent level. These estimates are consistent with the hypothesis that aggregate firm performance offers little marginal information to be used by firms in making dismissal decisions above and beyond what can be learned directly from divisional performance (i.e., aggregate firm performance is not informative).

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[7] The 4.11 percent increase in the likelihood of turnover is reasonably similar to the approximately 5 percent that Fee and Hadlock (2004) report for CEOs moving from the midpoint of the top stock return performance quartile to the midpoint of the bottom quartile.

[8] The model fit also appears reasonably good, as the actual turnover rate for managers with below-median predicted probability of turnover is substantially lower than the complement group (6.50 percent versus 10.39 percent). As a comparison, the corresponding figures for the CEO turnover models of Warner et al. (1988) are 10.1 percent and 12.8 percent.

TABLE 3
Logit Models of Division Manager Turnover

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Division ROA	−1.96**	−2.42**		−2.31**		−2.16*	−2.13**
	(0.92)	(0.96)		(0.96)		(1.14)	(0.97)
Industry ROA		8.51**		8.60**		8.60**	8.33**
		(3.35)		(3.40)		(3.35)	(3.32)
Industry Adj. ROA			−2.58***		−2.49***		
			(0.96)		(0.96)		
Counterpart ROA				0.42			
				(0.81)			
Counterp Ind. Adj. ROA					−0.06		
					(0.78)		
Firm ROA						−0.72	
						(1.73)	
One-Year Excess Stock Return							−0.48
							(0.31)
Age	0.04**	0.04**	0.04**	0.04*	0.04*	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	−4.15***	−4.75***	−4.25***	−4.83***	−4.24***	−4.73***	−4.80***
	(1.02)	(1.07)	(1.02)	(1.08)	(1.02)	(1.07)	(1.08)
Log-Likelihood	−279.71	−276.57	−278.24	−273.73	−275.49	−276.48	−268.18
Pseudo R^2	.017	.028	.022	.028	.021	.028	.032
Model Chi-Squared	9.68	15.97	12.62	15.47	11.93	16.13	17.74
Number of Observations	983	983	983	975	975	983	952
Actual Turnover Rates							
Pred. Prob. <= Median	7.52%	5.89%	6.50%	5.74%	6.56%	5.89%	5.66%
Pred. Prob. > Median	9.37%	11.00%	10.39%	11.09%	10.27%	11.00%	11.37%
Implied Probabilities							
Div. ROA at 10th percentile	10.22%	10.46%	10.85%	10.31%	10.73%	10.14%	10.08%
Div. ROA at 90th percentile	6.11%	5.54%	5.61%	5.62%	5.68%	5.76%	5.76%

(continued on next page)

TABLE 3 (continued)

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*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively.

All estimates are for logit models with asymptotic standard errors reported in parentheses under the coefficient estimates. The unit of observation is the division manager-year. The dependent variable assumes a value of 1 for all departures except those we classify as jumps or exogenous. The dependent variable assumes a value of 0 if there is no job change and missing (i.e., it is excluded from the regression) otherwise. Observations for managers aged 63 or older are excluded from the estimation. All performance variables are for the period ending immediately prior to the observation year. Industry-adjusted ROA is a division's ROA less the ROA of the firm's industry. All other variables are defined in the preceding tables. Actual turnover rates are for all observations that the model predicts will fall below or above the median level of turnover likelihood. The implied probabilities are calculated assuming that all variables except ROA are held constant at the sample mean. The model Chi-squared statistic is for the joint significance of adding all explanatory variables to an initial model that only includes a constant.

Robustness of Turnover Findings

To investigate the robustness of the preceding findings, in untabulated results we experiment with adding additional control variables (one at a time). In particular, we alter the baseline models in columns 2 and 3 of Table 3 by: (1) adding a year trend, (2) adding year dummies, (3) adding a control for division size, (4) adding a control for firm size (log of inflation-adjusted assets), (5) adding the fraction of the firm's sales derived from the division, and (6) adding a variable indicating whether the division has the same two-digit SIC code as the firm. When we add these variables, they are in all cases insignificant, while the coefficients on the performance variables change little in magnitude or statistical significance.[9]

Our initial models treat all observations as independent, even if individual managers remain in the sample for multiple years, as is frequently the case. Consequently, it is possible that we have understated the standard errors. To investigate this possibility, we consider several alternative econometric techniques to adjust the estimates and/or standard errors. We report these results for a baseline model corresponding to model 2 of Table 3. In column 1 of Table 4 we present estimates with standard errors that are robust to possible heteroscedasticity and serial correlation. In column 2 we calculate standard errors using the bootstrap procedure outlined by Efron and Tibshirani (1986). In column 3 we consider an alternative possible error structure by estimating a random effects probit model with individual random effects. This allows for the possibility that there is a person-specific job departure factor. Finally, in column 4 we include an individual's past tenure in the position as a control variable. As the table illustrates, in all four columns the estimates are substantively unchanged from what we report earlier.

An alternative approach for modeling turnover is to use survival models (see Kiefer 1988). This approach has the advantage that each individual career is treated as a single observation. We estimate a model of this type by using a discrete version of the Cox proportional hazards model estimated using the methods described by Kalbfleisch and Prentice (2002). A coefficient above 1 (below 1) in these models implies that the indicated variable shifts the hazard function upward (downward), thus indicating that the variable decreases (increases) average career length. As we report in column 5 of Table 4, the coefficient on division performance (industry performance) is significantly below 1 (above 1), indicating that superior performance relative to the industry is associated with retaining the division manager position for a longer time. This helps to further confirm the robustness of our turnover findings to alternative estimation techniques.

Alternative Performance Metrics

As we discuss earlier, it is not clear which performance metrics will best reflect a division manager's effort or ability. Our initial choice of ROA is drawn from the work of Blackwell et al. (1994). However, several other measures have been discussed in the literature. To investigate, we examine the effect of replacing ROA in the turnover models with various alternatives. In the interest of brevity, these estimates are not reported in the tables.

Some authors use changes in ROA as a measure of performance. When we use three-year or five-year changes in ROA (industry-adjusted ROA), the results corresponding to

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[9] In cases (4), (5), and (6) we also experiment with interacting the added variables with divisional performance, and in no case is the interaction term significant. We also experiment with including a control for whether the CEO recently departed. Consistent with Fee and Hadlock (2004), the coefficient on this variable is typically positive and significant, but its inclusion has no substantive effect on the performance coefficients.

TABLE 4
Alternative Models of Division Manager Turnover

	(1)	(2)	(3)	(4)	(5)
Estimation Method	**Logit, Robust Cluster**	**Logit, Bootstrap**	**Random Effects Probit**	**Logit, Conditional on Tenure**	**Discrete Cox Hazard**
Division ROA	−2.42***	−2.42***	−1.29***	−2.72***	0.12**
	(0.85)	(0.92)	(0.50)	(0.97)	(0.02)
Industry ROA	8.51***	8.51***	4.79**	8.63**	6020.66***
	(2.75)	(3.03)	(1.87)	(3.45)	(0.01)
Age	0.04*	0.04*	0.02*	0.03	1.03
	(0.04)	(0.02)	(0.01)	(0.02)	(0.12)
Constant	−4.75***	−4.75***	−2.62***	−5.18***	
	(1.07)	(1.15)	(0.54)	(1.18)	
Log-Likelihood	−276.57	−276.57	−276.51	−268.99	−272.01
Model χ^2	18.47	15.21	15.31	28.99	14.41
No. of Obs.	.028	983	983	971	983

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*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively.

The estimates in column 1 are maximum likelihood logit model estimates with standard errors that are robust to heteroscedasticity and are clustered by individual to account for potential serial correlation. The estimates in column 2 calculate standard errors using the bootstrap procedure outlined by Efron and Tibshirani (1986) and implemented in Stata using the vce(bootstrap) option with 200 repetitions. The estimates in column 3 are maximum-likelihood estimates for a random effects probit model with an assumed random effect for each division manager. The estimates in column 4 are logit estimates for a model that includes a control (estimate not reported) for the individual's tenure. For all columns except column 5 we report coefficient estimates with standard errors in parentheses. In column 5 we report the results of a discrete-time version of a Cox proportional hazards model. We report in this column hazard ratios with estimated p-values in parentheses. In the column 5 model each division manager career is treated as a single observation. Careers are assumed to end when a division manager departs (except for job jumps and exogenous changes). All other job changes are treated as right censored observations. All managers who remain in office at age 63 are treated as right censored after that point. In columns 4 and 5 we assume that managers who appear in the sample in the first sample year started in their position four years prior. In column 5 these observations are treated as left censored. The age variable in column 5 is the age of the individual when they assumed their division manager position.

model 1 (model 3) of Table 3 are qualitatively unchanged. In particular, poor divisional performance is associated with significantly elevated dismissal rates. However, when we use one-year changes in ROA or industry-adjusted ROA as the performance measure, the coefficients are insignificant. Thus, it appears that accounting performance levels and changes are both related to division manager turnover, provided that changes are measured over a sufficiently long period. The fact that recent changes in ROA are not significantly related to division manager turnover suggests that there may be substantial noise in annual fluctuations in profitability.

A high ROA in a large division will correspond to greater profit generation than a high ROA at a small division. Consequently, one might expect the sensitivity of turnover to ROA to be greater for individuals managing larger divisions. To investigate, we add to models 1 and 3 of Table 3 terms that interact divisional performance with a dummy variable indicating whether the division was the larger division of the firm. Alternatively, we interact ROA with the fraction of the firm's assets that are derived from the division less one-half. In both cases the sign on the interaction term is insignificant. Finally, if we use total division profits in place of ROA as our performance measure in model 1 of Table 3, the coefficient on this performance metric is not significant.

It is possible that firms evaluate managers based on growth rather than profits. This could arise if sales are a strong signal of managerial ability/effort or if firms want to strategically commit to aggressive behavior in the product market (e.g., Katz 1991). To investigate this possibility, we estimate models corresponding to columns 1–3 of Table 3 by replacing the performance metrics with divisional and industry three-year sales growth rates. In all cases the coefficients on these alternative performance variables are insignificant. Thus, we find little evidence indicating that growth by itself is rewarded in the turnover process.

Sample Characteristics and Turnover

As we mention above, the special features of our sample may affect the generality of our findings. To explore this issue in more detail, we consider how turnover may vary with firm characteristics. Since firms that survive longer will tend to be over-represented in our sample, we first examine whether the turnover-performance relation is elevated for the longer-lived firms that survive into the latter part of our sample period. Specifically, in column 1 of Table 5 we estimate a logit model of turnover that includes a term interacting a division's industry-adjusted ROA with a dummy variable indicating whether an observation falls in the second half of the sample period. As the table illustrates, the coefficient on divisional performance in this model remains negative and significant ($p = 0.075$), while the interaction term is negative and insignificant ($p = .20$).[10] These estimates suggest that our findings are not entirely driven by studying a small set of durable firms.

The implementation of FASB Statement No. 131 (SFAS No. 131, *Disclosures about Segments of an Enterprise and Related Information*) was intended to increase the information content in segmental accounting disclosures starting in fiscal year 1998 (see Berger and Hann 2003). This raises the possibility that the turnover-performance relation is elevated after the new rules took effect. When we interact divisional performance with a post-1998 dummy in column 2 of Table 5, the sign is of the expected negative sign, but it is far from significant ($p = 0.36$). Thus, it does not appear that our main findings are driven by the post-1998 period.

Our sample selection rules require a clear and unambiguous mapping between managers, divisions, and segments. Thus, it is likely that we tend to select firms with stable divisional structures. This suspicion is confirmed by the data. Relative to the set of all two-segment Compustat firms in existence at the start of the sample period, sample firms display statistically significantly lower annual rates of changes in the number of reported segments (12.3 percent versus 21.1 percent) measured over the sample period. In addition, as of the start of the sample period, firms in our sample had a two-segment structure for a median of six years, which is statistically significantly greater than the median two-segment Compustat firm, which had this structure for only three years.

To consider the role of the stable nature of the divisional structure of sample firms on our inferences, we create a dummy variable that assumes a value of 1 for firms that had maintained the same number of segments for less than three years as of the start of the sample period. This variable should be an indicator of a firm with a less stable organizational structure. If divisional accounting metrics are relied on less when evaluating managers in these firms, then the interaction of this variable with performance should be positive in our turnover models. As we report in column 3 of Table 5, the coefficient on this interaction

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[10] The results in columns 1 and 2 of Table 5 are substantively unchanged if we also include a separate dummy variable for the latter subperiods. The main results of all models in Table 5 are also similar in character if we use the alternative estimation techniques employed in Table 4.

TABLE 5
Variation in Division Manager Turnover across Firms

	(1)	(2)	(3)	(4)
Industry-Adjusted ROA	−1.90*	−2.30**	−3.44***	−2.63***
	(1.07)	(0.99)	(1.25)	(0.98)
Industry-Adjusted ROA * Second Half of Sample Dummy	−3.00			
	(2.34)			
Industry-Adjusted ROA * Post-1998 Dummy		−3.09		
		(3.38)		
Industry-Adjusted ROA * Low Stability Dummy			2.56	
			(1.95)	
Counterpart Industry-Adjusted ROA				1.61
				(1.03)
Counterpart Industry-Adjusted ROA × SIC Overlap Dummy				−4.24**
				(1.73)
Age	0.04**	0.04**	0.04**	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)
SIC Overlap Dummy				0.60**
				(0.28)
Low Stability Dummy			0.60**	
			(0.26)	
Constant	−4.29***	−4.35***	−4.52***	−4.90***
	(1.01)	(1.02)	(1.04)	(1.10)
Log-Likelihood	−277.37	−277.82	−274.05	−271.81
Pseudo R^2	.025	.024	.037	.034
Model Chi-Squared	14.36	13.47	21.00	19.30
Number of Observations	983	983	983	975
Actual Turnover Rates				
Pred. Prob. <= Median	6.50%	6.30%	5.28%	5.53%
Pred. Prob. > Median	10.39%	10.59%	11.61%	11.29%

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively.
All models are logit models of turnover with the same sample and dependent variable as in Table 3. Asymptotic standard errors are reported in parentheses beneath the coefficient estimates. The second-half (post 1998) dummy variable assumes a value of 1 for observations from 1997 to 2000 (1999 and 2000). The low stability dummy assumes a value of 1 for all firms that had maintained a two-segment structure for less than three years as of the start of the sample period. The SIC overlap variable is a dummy variable coded as a 1 if the two divisions of the firm include segments with the same two-digit SIC code. All other explanatory variables are defined as in Table 3.

term is positive, but it is not significant (p = 0.191). At the same time, the coefficient on divisional performance remains negative and significant. We obtain similar results when we use six years rather than three years to define the dummy variable. Thus, we fail to uncover strong evidence that the sensitivity of turnover to performance is affected by the stability of an organization's divisional structure, increasing our confidence in the generality of the turnover findings.

Internal Benchmarking

In contrast to Blackwell et al. (1994), we find little evidence that a division manager's performance is measured relative to a firm's other divisions in turnover decisions. This difference may arise from differences in sample characteristics. In particular, the subsidiary banks in the Blackwell et al. (1994) sample are separate legal entities with distinct businesses under common ownership, whereas the divisions in our sample are likely to have less distinct boundaries and to be more interdependent. Thus, we suspect that divisions in our sample are more likely to sell some of their output to other divisions or to share common assets in the production process. To the extent that this is the case, strong performance at one division may be a positive signal for the manager of the other division. For example, if transfer-pricing rules artificially attribute much of the profits from intrafirm transactions to one of the two divisions, then strong performance at that division should be a positive signal for the other division. In addition, if a decision is made to allocate a disproportionately small share of the cost of a shared asset to one of the two divisions, then an inflated level of profitability at that division should be partially attributed to the other division.

To further investigate, we seek to identify firms where activities in one division may be partially reflected in the accounting performance of the other division. Following Bushman et al. (1995), it is reasonable to expect that divisions in the same two-digit SIC industry are more likely than others to sell output to each other and/or to share common assets. Thus, in these firms, we might expect division managers to be rewarded rather than penalized for strong performance at a counterpart division. This would imply a more negative coefficient on counterpart performance for these firms in a logit model of turnover.

In column 4 of Table 5 we estimate a model that allows the effect of counterpart performance on turnover to vary with an industry overlap dummy variable indicating whether the firm's divisions are in the same industry. As we hypothesized, the coefficient on the interaction term is negative and significant ($t = -2.45$), suggesting that managers in these firms are rewarded rather than penalized for strong performance at the counterpart division.[11] This evidence is consistent with the notion that within-firm relative performance evaluation can be costly or undesirable when internal units are interdependent.

V. PROMOTIONS

Models of Division Manager Promotions

We now turn to modeling promotion decisions. If strong performance is a signal of managerial effort or ability, we expect to observe a positive relation between performance and promotions. The dependent variable in these logit models assumes a value of 1 if a division manager is promoted during the year (grouping definite and probable promotions together), 0 if there is no job change, and missing for all other job changes. Since managers aged 63 or older are unlikely to be promoted, we exclude these individuals from the estimated models.

In column 1 of Table 6, a model in which a division's own ROA is the only performance measure, the coefficient on performance is positive but not significant at conventional levels ($t = 1.29$). Thus, in this initial model, we are unable to detect a significant relation between performance and promotions. In column 2 we add counterpart ROA (i.e., the other division's ROA) to the specification. In this model, the coefficient on own performance becomes significant at the 10 percent level, while the coefficient on counterpart performance has the expected negative sign but is not significant. These estimates offer little conclusive evidence.

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[11] The implied point estimates on counterpart performance for firms with divisions in non-overlapping (overlapping) industries is positive (negative) and significant at the 15 percent (10 percent) level. Interpreting interaction terms in logit models can be problematic (e.g., Powers 2005). Following Ai and Norton (2003), we calculate the estimated marginal effect on turnover of varying industry overlap at each sample point and the median Z-statistic is significant at the 5 percent level.

TABLE 6
Models of Division Manager Promotions: Main Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation Procedure	**Logit**	**Logit**	**Logit**	**Logit**	**Conditional Logit**	**Conditional Logit**
Division ROA	1.42	1.92*			9.48*	
	(1.10)	(1.11)			(4.88)	
Counterpart ROA		−0.18				
		(1.30)				
Dummy for Division ROA > Counterpart ROA			1.08***	1.32**		1.38***
			(0.42)	(0.56)		(0.50)
Division ROA − Counterpart ROA				−0.82		
				(1.25)		
Age	−0.04	−0.03	−0.03	−0.03	0.01	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)
Constant	−1.54	−2.27	−2.32*	−2.84*		
	(1.47)	(1.57)	(1.61)	(1.60)		
Log-Likelihood	−144.18	−133.76	−131.34	−131.13	−20.44	−18.85
Pseudo R^2	.013	.014	.032	.034	.137	.204
Model Chi-Squared	3.67	3.91	8.73	9.17	6.46	9.64
Number of Observations	934	924	924	924	69	69
Actual Promotion Rates						
Pred. Prob. <= Median	2.57%	2.16%	1.73%	1.73%	22.22%	17.14%
Pred. Prob. > Median	4.71%	4.55%	4.99%	4.98%	54.55%	58.82%
Implied Probabilities						
Div. ROA at 10th Percentile	2.86%	2.44%			21.94%	
Div. ROA at 90th Percentile	4.22%	4.15%			80.73%	
Div. ROA > Counterpart			4.90%	5.46%		79.93%
Div. ROA ≤ Counterpart			1.72%	3.94%		20.07%

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively.

The dependent variable in all models assumes a value of 1 if a division manager experiences a job change during the year that we categorize as a definite or probable promotion. The dependent variable assumes a value of 0 if there is no job change and missing (i.e., it is excluded from the regression) otherwise. Observations for managers aged 63 or older are excluded from the estimation. Asymptotic standard errors from the maximum likelihood estimation are reported under the coefficient estimates. Division ROA and Counterpart ROA (i.e., ROA of the other division) are for the year ending immediately prior to the observation year. The estimates in columns 1–4 are for standard logit models estimated over the set of all division-manager-years. The estimates in columns 5 and 6 are for conditional logit models restricted to cases in which one of the firm's division managers was promoted during the year. Actual promotion rates are for all observations that the model predicts will fall below or above the median level or promotion likelihood. Implied probabilities are calculated assuming that all variables except ROA are held constant at the sample mean.

As we discuss earlier, some theories posit that managers are promoted primarily based on ordinal rankings of performance. To investigate, in column 3 of Table 6 we include a dummy variable indicating whether a division's ROA exceeds the ROA of the division's counterpart. The coefficient on this variable is positive and highly significant ($t = 2.59$), and the magnitude of the effect appears to be substantial. For a manager at a division with performance below the other division, the implied likelihood of a promotion in any given year is 1.72 percent. This figure almost triples to 4.90 percent if the division is the superior performer at the firm.

To further investigate this finding, we include in column 4 both the dummy variable indicating superior within-firm relative performance and a variable measuring the magnitude of the performance difference. The coefficient on the dummy variable remains positive and highly significant, while the coefficient on the magnitude of the performance difference has a negative sign (opposite of expected) and is insignificant. This evidence is consistent with the tournament theory prediction that promotion decisions will be based on ordinal rankings of performance rather than the actual level of a manager's performance.

Since job openings may arrive infrequently, the models we estimate above may not present a complete picture of promotion behavior. Thus, we also estimate conditional logit models of promotions. This approach restricts attention to observations in which one of the firm's division managers is promoted and estimates a model predicting which manager is chosen. As we report in column 5 of Table 6, when we include divisional ROA as the performance metric in a model of this type, the coefficient is positive and significant at the 10 percent level. However, when we instead use as our performance metric a dummy variable indicating superior within-firm relative performance, as in column 6, the coefficient is positive and highly significant (t = 2.79). This is again consistent with the hypothesis that ordinal rankings of performance are closely related to the promotion decision. The estimated magnitude of this effect appears to be substantial. Using the column 6 estimates, the estimated likelihood that a poorer performing division manager is chosen for a promotion when an opportunity appears is only 20.07 percent, while for the superior performer this figure is 79.93 percent.

Alternative Performance Metrics and Robustness

We now consider the role of alternative performance metrics in the promotion decision. When we use changes in ROA (one-year, three-year, or five-year) in place of the level of ROA, the coefficients on divisional performance in models corresponding to those of Table 6 are generally insignificant and sometimes of the wrong sign. This suggests that promotions are more related to the level of accounting performance than to recent changes. When we add industry performance to the Table 6 models, the coefficients on these variables are in all cases insignificant. Thus, we find no evidence that industry performance is an important component in the promotion decision.[12]

To consider the role of division size on our findings, we modify the models in Table 6 in an analogous way to the modifications in the turnover analysis. These estimates reveal no significant difference in the promotion-performance sensitivity as division size changes. In addition, when we replace the dummy variable for having a greater profit rate than the counterpart division with a dummy variable indicating a greater profit level, the coefficient is in all cases positive but insignificant. We also check whether pure growth is rewarded in the promotion process by adding three-year sales growth terms to the Table 6 models and also by replacing the ROA-based variables with their sales growth-based counterparts. In all cases the coefficients on the sales growth variables are insignificant.

To check the robustness of the promotion findings, we perform robustness checks analogous to what we report earlier for the turnover results. In particular, we experiment with adding a full set of additional control variables and using alternative estimation techniques.

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[12] The fact that turnover and promotions are not associated with the exact same performance metrics suggests that firms are solving different problems when they determine their policies regarding these two types of job changes.

The result that the division manager with the higher ROA is significantly more likely to be promoted is substantively unchanged under all of these alterations.

An Auxiliary Sample

The key finding in our preceding analysis is that managers in two-division firms are substantially more likely to be promoted when their ROA performance exceeds that of their internal counterpart. This evidence helps complete an important part of the picture on promotions of senior executives. However, given the special features of our sample coupled with the small number of observed promotions, one may be concerned about the generality of this finding. Thus, in this subsection, we examine an alternative auxiliary sample.

To construct this sample, we screen the names and titles of all executives on the Compact Disclosure CDs from 1992 to 2006 (two disks per year). We identify every case in which an executive with the title of president, COO, or CEO was listed in the previous year with the title of division or group president. We then examine corporate filings to identify whether and when the individual jumps directly from a division-manager-level position to one of these top corporate positions. Finally, we compare the name of the division run by the manager and the segment listings on Compustat to see if a clear and unambiguous matching can be made. We retain in the auxiliary sample all promotion events not in the original sample that pertain to a manager under the age of 63 who can be matched to a segment of a multisegment firm. The resulting sample contains 61 promotions.[13]

For each promotion, we know the identity and segment(s) of the promoted manager. In addition, we know that managers of the firm's other segments were not selected for the promotion. However, the identity of these other managers is not known. Thus, while we have information on division characteristics for other divisions, we do not have information on individual manager characteristics (e.g., age). This sample should be substantially more representative of the general population of multidivisional firms since it has no survivorship features and includes a longer time period, firms with more than two divisions, and divisions with substantially differing sizes.

In column 1 of Table 7 we present a conditional logit model predicting promotions in the auxiliary sample with divisional ROA as the sole explanatory variable. Similar to our findings for the original sample, the coefficient on raw performance is positive but insignificant. Turning to the role of within-firm relative performance, in column 2 we replace the ROA variable with a dummy variable indicating whether a division had the highest ROA among the firm's divisions. If firms rely heavily on performance rankings when making promotion decisions, we expect to observe a positive coefficient on this variable. As we report in the table, the coefficient on this variable is in fact positive and significant ($p = 0.05$). Moreover, the economic magnitude of the effect appears substantial. In a firm with two divisions, the estimates imply that the superior (inferior) performer has a 63 percent (37 percent) chance of being selected for promotion.

The auxiliary sample contains some divisions that are much larger or smaller than other divisions in the same firm. Thus, it seems reasonable to suspect that division size may be an important factor in the auxiliary sample, since managers of larger divisions may tend to be better candidates for promotion. To account for this possibility, we add in column 3 of Table 7 a control for division size. As expected, the size coefficient is positive and significant. More importantly, the coefficient on the highest divisional ROA indicator variable

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[13] Clearly our screening procedure will miss many promotions. However, it is a practical approach to identify unambiguous promotions in order to examine some general features of promotion decisions in a conditional context.

TABLE 7
Conditional Logit Models of Promotions: Alternative Samples

Sample	**(1) Auxiliary Sample**	**(2) Auxiliary Sample**	**(3) Auxiliary Sample**	**(4) Combined Sample**	**(5) Combined Sample**
Division ROA	0.12				
	(0.44)				
Highest Divisional ROA in Firm Dummy Variable		0.53**	0.75**	0.98***	0.96***
		(0.27)	(0.36)	(0.28)	(0.37)
Highest Divisional ROA Dummy * Post-1998 Dummy					0.04
					(0.54)
Log(division assets)			1.47***	1.16***	1.16***
			(0.28)	(0.22)	(0.22)
Log-Likelihood	−73.27	−71.45	−44.10	−68.85	−68.85
Pseudo R²	.001	.025	0.387	0.280	0.280
Model Chi-Squared	0.07	3.71	55.76	53.61	53.62
Number of Observations	220	220	216	285	285
Implied Probabilities					
Highest Divisional ROA = 1		63.00%	68.00%	72.72%	
Highest Divisional ROA = 0		37.00%	32.00%	27.28%	

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*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively.
The auxiliary sample (columns 1–3) includes a broad set of firms derived from the Compact Disclosure CDs between 1992–2006. The combined sample (columns 4–5) includes the auxiliary sample and the original sample. All coefficient estimates are for conditional logit models with asymptotic standard errors reported under the coefficient estimates. The dependent variable assumes a value of 1 for the promoted executive and his/her associated division and 0 for the segments of the firm not associated with the promoted executive. Division ROA is the ROA of the segment in the fiscal year immediately prior to the fiscal year in which the promotion took place. The highest ROA dummy variable assumes a value of 1 if the ROA of the division exceeded all other divisions and 0 otherwise. Implied probabilities are calculated for a hypothetical two-division firm using the estimates in each column with all variables fixed at the sample mean except for the dummy variable indicating top divisional performance at the firm.

remains positive and significant in this model. We obtain similar results for the same specification in column 4, which is estimated over the combined set of promotions from both samples. This evidence indicates that our earlier findings on promotions are robust to concerns regarding sample composition.

The combined sample of promotions includes a substantial number of observations from both before and after the implementation of SFAS No. 131. Thus, we consider the possible effect of this change by adding a term to the promotion models that interacts the performance variable with a post-1998 dummy variable. As we report in column 5, the estimated coefficient on this term is positive but insignificant. Thus, we find little evidence of a significant effect of this rule change on the relation between accounting figures and promotion decisions.

VI. CONCLUSION

Our understanding of how executives below the CEO are evaluated and rewarded in the internal labor market is limited. Using information on job changes of division managers, we find a strong negative relation between the likelihood that a division manager will depart from her employer and divisional ROA. This evidence supports the hypothesis that accounting information is used in evaluating managerial personnel. Firms appear to adjust this information by comparing managers to industry benchmarks when making turnover decisions, supporting the industry RPE hypothesis. However, for turnover we find little convincing evidence that managers are compared to internal counterparts at other divisions within the firm, casting doubt on the within-firm RPE hypothesis in the context of turnover decisions.

Promotions are only weakly related to the level of a division's performance, but they are strongly positively related to whether one division has better performance than the firm's other units. These findings are consistent with the presence of a tournament aspect to promotions, where managers are evaluated based on ordinal rankings of performance. The incentive structure induced by promotion decisions may create an element of competition between managers. However, if one division is far outperforming the other, then promotion-based incentives may become weak, since a reversal in performance rankings would then be unlikely. Our direct evidence on the presence of tournament incentives complements the findings of Kale et al. (2009), who report that tournament incentives, which they measure only indirectly, have an important effect on firm performance.

Our findings on turnover and promotions support the general notion that internal labor markets can have a meaningful impact on managerial incentives near the top of the corporate hierarchy. While our evidence helps answer some questions regarding incentives and performance evaluation for executives below the CEO, it also leads to further questions. In particular, since compensation systems, turnover policies, and promotion policies all appear to emphasize different measures of performance, it would be interesting to assess the net effect of all of these devices on managerial incentives.

Finally, in our analysis we deliberately choose to study positions for which easily identifiable measures of individual performance are generated by the accounting system. However, for many managerial positions, quantifiable measures of individual performance are not as readily apparent. It would be interesting to understand how managerial talent is evaluated for these types of positions. For example, are subjective evaluations of individual performance relied on more heavily in these settings? These and related questions await future research.

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Knowledge Sharing and Incentive Design in Production Environments: Theory and Evidence

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ABSTRACT: We develop and empirically test a parsimonious model of how specific knowledge and the value of knowledge sharing influence manufacturing plants' incentive design choices. Our results confirm the prediction that increases in the extent of agents' specific knowledge and the value of knowledge sharing are associated with greater (less) reliance on output (input) performance measures. Moreover, consistent with our model's prediction, we find that plants rely more on group-based (as opposed to individual-based) output performance measures when the value of knowledge sharing is higher, or the extent of agents' specific knowledge is lower. Finally, consistent with previous research, we find that as output performance measures become noisier, firms rely less on these measures in incentive contracts.

Keywords: *specific knowledge; knowledge sharing; incentive design; group incentives.*

Data Availability: *Contact IndustryWeek (http://www.industryweek.com) with questions concerning data availability.*

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I. INTRODUCTION AND PRIOR RESEARCH

Firms design compensation systems, and more broadly human resource systems, to attract and retain certain types of employees, and to give them incentive to provide the level and mix of services the firm prefers. Over the last 25 years, one response of U.S. manufacturing firms to increased competition has been to introduce new compensation policies that generally replace fixed hourly pay for production employees with variable compensation. Under the new compensation arrangements, some production workers are paid exclusively based on their individual input (e.g., number of hours worked), while others receive both pay based on input measures and also compensation based on their individual and/or group output. To explain this rich variety of compensation structures, we develop and test a parsimonious model of optimal production incentives.

We find considerable diversity in how the 1,780 U.S. manufacturing plants in our sample compensate their employees. Approximately one-half of all sample plants determine pay using only input performance measures, while the remaining plants use a combination of input and output measures (Panel A of Table 1). Further, the latter group is divided approximately evenly among plants using exclusively individual-based output performance measures, plants using a combination of individual-based and group-based output performance measures, and plants using exclusively group-based output performance measures.

The theme of our paper is that a significant portion of the preceding diversity in production employees' compensation can be explained by the extent of employees' specific knowledge and the value created when employees share this knowledge. Here specific knowledge is information that pertains to production employees' decision-making but is very costly to communicate to management (Jensen and Meckling 1992).

To address the role of specific knowledge and how it is shared in manufacturing environments, we extend Raith's (2008) one-agent, two-task model by introducing a second agent and allowing each agent to have private information on the productivity of both individual and group tasks. The agents' observable and contractible efforts across two tasks, together with exogenous state uncertainty, determine the principal's total output. The agents can improve their group-task-related knowledge by sharing their information, and when the principal uses output performance measures to determine the agents' pay, this gives the agents incentive to utilize their specific knowledge to benefit the principal. Although pay based on output performance measures is typically riskier than pay based on input performance measures, the cost of this additional risk may be offset by the potential gains from exploiting the agents' specific knowledge.

We first derive predictions from this model regarding the choice of output performance measures and the incentive weight placed on these performance measures as a function of the plant's technology and operating environment. We then test these predictions using data from a national survey of manufacturing plants. Our empirical tests are based on proxies for the plant's technology and operating environment, as well as controls for a variety of other plant characteristics.

Prior research has addressed the trade-offs involved in rewarding agents based on input versus output performance measures, and the corresponding trade-offs between using measures of individual versus group output. However, this work typically focuses on one of the two trade-offs, whereas our model incorporates both. Prendergast (2002) demonstrates when optimal contracts will shift from using less risky input measures to using riskier output measures in order to motivate agents to use their specific knowledge. Hence, the value of agents' private information can increase in more uncertain environments and, consequently, delegation and output performance measures will be observed more frequently in higher risk environments. Consistent with this view, Ortega (2009) finds a positive association between more complex job environments in which employees have more extensive specific knowledge and measures of employee discretion and output-based pay.

Other prior research documents that it is common in modern production environments to reward production workers based on both individual outputs and on group outputs. For example, in their study of 249 firms in the automotive and computer industries, Ittner and Larcker (1995) find that, although individual performance tends to outweigh team performance in determining compensation, the mean (median) ratio of team performance importance to individual performance importance was 0.797 (0.750). Further, Shaw and Schneier (1995) find that in *Fortune* 100 companies, rewards for individual performance remain important even when firms rely heavily upon teams.

Existing theory offers diverse explanations for the use of group-based output measures. Agency literature on moral hazard (Holmstrom 1982; Holmstrom and Milgrom 1994) establishes that group-based incentives can create valuable externalities, even when agents have limited decision-making discretion, because group-based output measures can provide additional contracting information beyond that provided by individual-based performance measures. Consistent with this prediction, Bushman et al. (1995) find that the use of aggregate or group performance measures in business unit managers' compensation contracts is an increasing function of divisional interdependencies.

In contrast to these studies, we emphasize how specific knowledge and the value of sharing this knowledge play important roles in the choice between individual-based output and group-based output measures. Prior literature demonstrates that when specific knowledge is dispersed across individuals, assigning decision rights to a team of employees rather than to individual employees can improve the joint allocation of decision rights and specific knowledge (Wruck and Jensen 1994). For example, Keating and Wruck (1993) report how the implementation of a TQM program at Sterling Chemicals succeeded because quality teams elicited the specialized knowledge of clerks, craftsmen, and machine operators to overcome significant operational bottlenecks. Similarly, Sprinkle and Williamson (2004) describe how John Deere Corporation shifted from individual-based incentives to group-based incentives to encourage greater information sharing, cooperation, and motivation among employees.

This study makes three contributions to the literature on management control systems and incentive system design. First, whereas previous studies have generally focused on the informativeness of performance measures (e.g., Murphy 2000; Bushman et al. 1995) and documented settings in which noisier signals will be weighted less heavily in incentive arrangements, we demonstrate that in other settings the optimal incentives based on firm outputs (noisier signal) become sharper when agents have specific knowledge about the state of the firm's production technology. Second, prior studies that investigate the influence of specific knowledge on incentive design (e.g., Raith 2008; Ortega 2009) have not addressed the role of knowledge sharing, an issue of particular importance in today's competitive environment. Therefore, by modeling the roles of specific knowledge and the sharing of this knowledge as related but distinct concepts, our analysis complements and extends such research on firms' choices between individual-based and group-based incentives.

Finally, our sample of 1,780 plants spans all the two-digit manufacturing SIC codes, which gives our study greater generalizability than previous research that relies on either relatively small samples (e.g., Bushman et al. 1995) or results from a single industry (e.g., Ichniowski et al. 1997).

Section II develops a two-agent model in which both agents take observable actions and have specific knowledge they can share. Section III develops testable hypotheses based on the model in Section II, and describes our empirical tests. The results of our empirical tests are presented in Section IV, and Section V reports the results of several robustness tests. Finally, Section VI presents a summary and conclusions.

II. THEORETICAL FRAMEWORK

Consider a manufacturing setting in which the principal employs two workers, each of whom allocates her total effort over two tasks: individual and group production. Worker i devotes effort $a_i \in R^+$ to producing individual output (Y_i) and effort $a_{iG} \in R^+$ to producing group output (Y_G).

Production

Total output Y is the sum of individual and group output, $Y = \Sigma_i Y_i + Y_G$ where $i = 1, 2$. Outputs Y_i and Y_G are either high (1) or low (0), as a function of the efforts (a_i, a_{iG}) and related productivity parameters, θ_i, θ_G. In particular, the probability of high individual output ($Y_i = 1$) is $min\{a_i\ \theta_i,\ 1\}$, and the probability of high group output $Y_G = 1$) is $min\{(a_{iG} + a_{jG})\ \theta_G,\ 1\}$.

Production Technology

The productivities of the plant's individual and group production technologies are $\theta_i = \bar{\theta}(1 + \tau_i)$ and $\theta_G = \bar{\theta}(1 + \tau_G)$, where $\bar{\theta}$ is a scaling parameter and τ_i, $\tau_G \in \{1, -1\}$ are indicator variables (*High* = 1, *Low* = −1) for true, unobservable productivity. Each worker receives two private signals, $\{s_i, s_{iG}\}$, where s_i, $s_{iG} \in \{1, -1\}$, that are potentially informative about the true productivity of the two tasks. We assume that workers cannot communicate their private productivity signals (specific knowledge) to the principal. The prior probability of high and low productivity is: $p(\tau = 1) = p(\tau = -1) = 1/2$.

Specific Knowledge and Knowledge Sharing

Following Raith (2008), we define the following information structure for each worker's individual production task: $p(s_i|\tau_i) = p(\tau_i|s_i) = (1 + \tau_i s_i k)/2$, where τ_i, $s_i \in \{1, -1\}$. Parameter $k \in [0,1]$ represents the extent of workers' specific knowledge in a given plant. As $k \rightarrow 0$, an agent's specific knowledge becomes less informative, and the posterior equals the prior of 1/2. As k increases, the worker's specific knowledge becomes more informative about the plant's production technology. For $k > 0$, when $\tau_i = s_i$ (i.e., both are +1 or both are −1), indicating "correct" beliefs, the worker's posterior increases above 1/2 and the magnitude of the increase is proportional to the strength of the agent's specific knowledge (k). When $s_i \neq \tau_i$ (i.e., an agent's signal does not match the actual state), the worker's posterior is < 1/2, again in proportion to the strength of the worker's specific knowledge (k).

For the group task we assume that workers can costlessly pool their private signals (s_{iG}, s_{jG}).[1] The information structure with pooled signals follows the same general structure as that of the individual task: $p(s_{iG}, s_{jG}|\tau_G) = \frac{1}{4}\left[(1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG})\tau_G \rho\right]$, where

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[1] We assume that specific knowledge can be communicated among production workers at no cost, but is prohibitively costly to communicate to management. This assumed cost differential reflects the fact that employees operating in the same physical location will naturally observe a variety of common signals concerning conditions of the plant in terms of temperature, humidity, availability of inputs such as raw material, etc. Because production employees share common observations, the cost of communicating each individual's unique information to other production employees will be significantly reduced. Conversely, because upper management cannot observe the common plant conditions, the cost of information sharing with management becomes prohibitive.

$\rho \in [0, 1 - k]$ reflects the value of knowledge sharing.[2] The assumptions above imply a joint probability of 1/4 for all combinations of the two signals (s_{iG}, s_{jG}) and the following posterior probability:

$$p(\tau_G|s_{iG}, s_{jG}) = \frac{1}{2}\left[(1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG})\tau_G \rho\right]$$

where $\tau_G \in \{1, -1\}$ and $s_{iG}, s_{jG} \in \{1, -1\}$ (see the Appendix for details). When the workers' signals are consistent (i.e., $s_{iG} = s_{jG}$), workers' posterior beliefs increase above 1/2 in proportion to the sum of the extent of specific knowledge (k) and the value of knowledge sharing (ρ). For example, when both signals are high (i.e., $s_{iG} = s_{jG} = 1$) we have: $p(\tau_G = 1|s_{iG} = s_{jG} = 1) = (1 + k + \rho)/2$. Hence, as $\rho \to (1 - k)$, pooling the two signals resolves all technological uncertainty and the corresponding posterior probability converges to 1. In contrast, as $\rho \to 0$, the informativeness of workers' combined signals reduces to that in the individual task where each worker relies on his own signal.[3] Moreover, when the group productivity signals are inconsistent (e.g., $s_{iG} = 1$, $s_{jG} = -1$), sharing of information adds no additional value and the conditional posterior probability converges to that with a single individual private signal $\frac{(1 + s_{iG}k\tau_G)}{2}$ (i.e., $p(\tau_G|s_{iG} \neq s_{jG})$).

Consequently, we model ρ and k as distinct, but related, concepts. Specific knowledge (k) captures the informativeness of employees' knowledge concerning the productivity of both individual and group tasks, while ρ is applicable only in group tasks and reflects the extent to which, by sharing their signals, employees can enhance the overall value of their individual knowledge, thereby reducing the uncertainty associated with the group task. In practice, the value of knowledge sharing (ρ) is often high when workers' have more extensive specific knowledge (k). For example, in project teams a diverse group of specialized workers (high k) may be temporarily assigned to a complex project that is too complicated for workers to solve on their own (high ρ). However, in other settings workers' specific knowledge may be high, while the value of knowledge sharing is low, or *vice versa*. For example, in a traditional manufacturing setting, workers are often assigned to a particular machine for months or even years at time, which makes them intimately familiar with their narrowly defined tasks (high k). To the extent that workers focus only on their narrowly defined job responsibilities, there is relatively less value to sharing knowledge in this environment.

Further, workers' specific knowledge may be relatively low in other settings in which workers are permanent members of self-contained, identifiable work units that conduct repetitive operations. However, in such settings, TQM teams can significantly increase productivity by encouraging workers to pool the information they encounter during their daily routines (high ρ). For example, Keating and Wruck (1993) describe an interview with a manager at Sterling Chemicals who stated that:

> The quality process works ... People must be willing to work together, willing to break down barriers. We have to accept the fact that much of the expertise resides in the

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[2] Note that our assumption that ρ can be larger than k allows for the possibility that agents learn new information by working together. Therefore, ρ can be interpreted as capturing the increase in group-task-related information stemming from the sharing and creation of knowledge when agents work together.

[3] We thank an anonymous reviewer for suggesting an information structure in which the information-sharing model converges to the single-signal model without information sharing as the information-sharing parameter (ρ) approaches zero.

> people who are doing the work. ... Previous management put together a group of chemical engineering Ph.D.s to find a way to get the lactic acid concentrator to work better. They couldn't do it. Solutions identified by one of our quality teams saved us \$2.5 million per year. The ideas came from hourly workers who spend every day of the week working with the process, not from engineers.

Workers' Utility

Workers are risk-neutral with utility $w - d(a_i, a_{iG})$, where w is compensation and the disutility of action is $d(.) = d(a_i^2 + a_{iG}^2)$. We assume that workers have limited liability such that their compensation is always positive, and that the participation constraints are such that we can set fixed salary at zero.[4] In addition, although linear contracts are not necessarily optimal in this setting, for tractability we assume that each worker's compensation is linear in three contractible measures: $w = \beta A_i + \gamma_i y_i + \gamma_G y_G$ where:

(1) a_i, and a_{iG} are individual input performance measures with associated payment of β per unit of input. We assume that the principal has perfect information on each worker's total effort $A_i = a_i + a_{iG}$, but not on specific effort components;
(2) y_i is the individual output performance measure with associated payment γ_i; and
(3) y_G is the group output performance measure with associated payment γ_G.

Worker i's utility function becomes:

$$U(A_i) = w(A_i) - d(a_i, a_{iG}) = \beta A_i + \gamma_i y_i + \gamma_G y_G - d(a_i^2 + a_{iG}^2). \tag{1}$$

Performance Measurement

Output performance measures (y) are related to the corresponding actual outputs (Y) through a common noise term $e \in [0,1]$. Specifically, the probability that $y_i = Y_i$ is $p(y_i = Y_i) = \frac{(2 - e)}{2}$, where $e \in [0,1]$. Similarly, $p(y_G = Y_G) = \frac{(2 - e)}{2}$. Larger values of the common noise term "e" reflect greater measurement error.[5]

Design Choices

Several design features of our model should be noted. First, to focus on specific knowledge and knowledge sharing, we assume that the principal has perfect information on each worker's total input (i.e., total effort (A_i) is perfectly observable) but not on measures of output. This assumption shifts the emphasis from the traditional moral hazard problem to a delegation problem in which workers have specific knowledge that is essential for productivity. Second, the principal's ability to observe workers' total effort, but not the distribution of total effort across the individual and group tasks reflects, for example, the routine tracking of employees' total hours worked but not the time devoted to each task.[6]

Finally, prior studies that have explored the relation between specific knowledge and the use of individual- versus group-based output performances have not addressed the role of knowledge sharing. Instead, these studies have argued that specific knowledge is more likely to be related to group-tasks than to individual tasks (Raith 2008, 8–9; Ortega 2009), or that specific knowledge is positively related to the diversity of tasks that employees perform, and therefore positively (negatively) associated with the use of group-based (individual-based) output performance measures (Ortega 2009, 607). Our approach is to explicitly model both specific knowledge and knowledge sharing as distinct characteristics of the firm's environment. This enables us to derive predictions that distinguish the effect of these two characteristics, particularly in our Proposition 3.

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[4] There is no loss of generality in assuming in the model that salary is equal to zero.

[5] Agents can potentially use their specific knowledge to game performance measures. See Indjejikian and Matějka (2008) for further development of this argument.

[6] Allowing the principal to have perfect information on the two individual effort components makes the incentive weight placed on the individual output performance measure independent of the value of knowledge sharing. All other conclusions drawn from our analysis remain unchanged.

Time Line

Principal chooses incentive weights $\{\beta, \gamma, \gamma_G\}$	Agents receives private signals $\{s_i, s_{iG}\}$	Agent chooses effort levels $\{a_i, a_{iG}\}$	Productivity Parameters realized $\{\tau_i, \tau_{iG}\}$	Final outcomes and payoffs realized and reported $\{Y_i, Y_G, y_i, y_G\}$

Optimal Incentive Contract

To solve for workers' optimal effort levels, we first provide the following expression for the expected value of individual performance measure y_i conditional on (θ_i, a_i):

$$E[y_i(\theta_i, a_i)] = \left(\frac{2-e}{2}\right) \times (\theta_i a_i) + \left(1 - \frac{2-e}{2}\right) \times (1 - \theta_i a_i) = \frac{e}{2} + (1-e) \times \theta_i a_i. \quad (2)$$

Similarly, the expected value of y_G (i.e., group performance) conditional on actions a_{iG} and a_{jG}, and the group productivity parameter $\theta_G \in [0,1]$, is:

$$E[y_G(\theta_G, a_{IG}, a_{jG})] = \frac{e}{2} + (1-e) \times \theta_G(a_{iG} + a_{jG}). \quad (3)$$

Hence, conditioned on private signals (s_i, s_{iG}, s_{jG}), worker i's expected utility is:

$$E[U(a_i)|s_i, s_{iG}, s_{jG}] = \beta(A_i) + \gamma\left[\frac{e}{2} + (1-e)\hat{\theta}_i a_i\right] + \gamma_G\left[\frac{e}{2} + (1-e)\hat{\theta}_G(a_{iG} + a_{jG})\right] - d(a_i^2 + a_{iG}^2) \quad (4)$$

where:

$$\hat{\theta}_i = E[\theta_i|s_i] - \bar{\theta}[1 + s_i k] \text{ and } \hat{\theta}_{iG} = E(\theta_G|s_{iG}s_{jG}) = \bar{\theta}\left[1 + s_{iG}k + \frac{(s_{iG} + s_{jG})}{2}\rho\right]. \quad (5)$$

The first-order conditions on worker i's actions a_i^* and a_{iG}^* (individual and group tasks) become:

$$a_i^* = \frac{\beta + \gamma(1 - e)\hat{\theta}_i}{2d}; \quad a_{iG}^* = \frac{\beta + \gamma_G(1 - e)\hat{\theta}_{iG}}{2d}. \tag{6}$$

Denoting the total output as $Y = \Sigma_i Y_i + Y_G$ and worker's private signals as $S = \{s_i, s_j, s_{iG}, s_{jG}\}$, the principal's expected payoff conditional on the productivity parameter $\theta = (\theta_i, \theta_j, \theta_G)$ and agents' private signal S is:

$$E(\pi) = E(Y(\theta, S)) - \sum_i w_i(\theta, S) = E\Bigg(\sum_i Y_i(\theta_i, S) + Y_G(\theta, S) - (\beta(A_i + A_j)$$
$$+ \gamma E(y_i(\theta, S) + y_j(\theta, S)) + 2\gamma_G E(y_G(\theta, S)))\Bigg). \tag{7}$$

Using the expected productivities (5) in Equation (6) and substituting the resulting expression, together with Equations (2) and (3) into the principal's *ex ante* profit function (7), we next take the expectations over the realizations of the information signals and productivity parameters (s_i, s_j, s_{iG}, s_{jG}, π_i, π_j, π_G) and optimize. See Equation (A2) in the Appendix for the resulting optimal incentive weights, β, y_i, and y_G.

Model Predictions

We first analyze the influence of k, the extent of manufacturing workers' specific knowledge, on the optimal incentive contract. More precisely, we derive the effect of k on the incentive weights that the plants place on input and output-based performance measures (all proofs are in the Appendix).

Proposition 1: As the extent of workers' specific knowledge (k) increases, the optimal weight on the input performance measure decreases ($\partial\beta/\partial k < 0$), and the optimal weights on both output performance measures increase ($\partial\gamma/\partial k > 0$ and $\partial\gamma_G/\partial k > 0$).

Proposition 1 parallels Raith's (2008) result that more specific knowledge calls for a greater weight on output performance measures (γ and γ_G). Therefore, even with perfect input performance measures, noisy output performance measures receive increasing weight because they induce workers to utilize their specific knowledge and thereby align workers' interests with that of the principal.

Next, we examine the effect of the value of knowledge sharing (ρ) on the optimal incentive weights placed on input and output performance measures. Larger values of ρ reflect environments in which workers who pool their private signals can make relatively more precise inferences concerning group productivity, θ_G.

Proposition 2: As the value of knowledge sharing (ρ) increases, the weight on the input performance measure decreases ($\partial\beta/\partial\rho < 0$), while the weights on both output performance measures increase ($\partial\gamma/\partial\rho > 0$ and $\partial\gamma_G/\partial\rho > 0$).

Proposition 2 establishes that as the value of knowledge sharing (ρ) increases, the optimal incentive weights will decrease on input measures and increase on output measures. The increase in the incentive weight on group-based output (γ_G) follows directly because an increase in ρ means that the workers are now pooling more diagnostic signals, which increases the expected productivity of effort on the group task.

The result that the weight on individual output (γ) should also increase is less direct. The shift in the optimal incentive mix toward more weight on the group-based output performance measures (γ_G) also reduces the input incentive weight (β). However, because β is the incentive weight on effort on both the individual and group tasks (recall that the principal observes only total effort, not the components), reducing β also indirectly reduces the agent's incentive to work on the individual task. Therefore, the principal must increase the weight on the individual-based output performance measures to compensate for the reduced input incentive weight, β. Hence, an increase in the value of knowledge sharing (ρ) results not only in greater incentive weight on group-based output performance measures (γ_G), but also greater incentive weight on individual-based output performance measures (γ). This leads to the interesting question of the relative influence of the value of knowledge sharing (ρ) and the extent of workers' specific knowledge (k) on individual- versus group-based output performance measures (γ, γ_G), which we address next.

Proposition 3: The relative weight placed on group- versus individual-based output performance measures is decreasing in workers' specific knowledge (i.e., $\partial(\gamma_G - \gamma)/\partial k < 0$ if $k > 1/2$) and increasing in the extent of the value of knowledge sharing (i.e., $\partial(\gamma_G - \gamma)/\partial\rho > 0$).

Proposition 3 characterizes the influence of the extent of specific knowledge (k) and the value of knowledge sharing (ρ) on the relative incentive weights placed on group (γ_G) versus individual-based output performance measures (γ). The intuition behind Proposition 3 comes from the differential impact that increases in specific knowledge (k) and the value of knowledge sharing (ρ) have on the returns to working on the individual and the group tasks. In particular, while more specific knowledge (k) directly increases the returns to both the individual and the group tasks, the magnitude of the gain to an individual employee from working on the group task is smaller because the gain is shared with all group members. Consequently, increases in the extent of workers' specific knowledge (k) shift the relative incentive weight from group-based output performance measures (γ_G) toward individual-based output performance measures (γ).

Further, while the increase in the incentive weight on group-based output performance measures (γ_G) due to an increase in the value of knowledge sharing (ρ) is a direct response to the increased productivity of the group task, the corresponding increase in the incentive weight on the individual-based performance measures (γ) represents an indirect, smaller response as described earlier. Hence, increases in the value of knowledge sharing (ρ) shift the relative incentive weight from individual-based output performance measure (γ) toward group-based output performance measure (γ_G).

Finally, Proposition 4 establishes how the quality of the output performance measure (e) affects the relative weight placed on input versus output performance measures.

Proposition 4: As the noise in the output performance measure (e) increases, the weight on the input performance measure increases ($\partial\beta/\partial e > 0$) and the weights on both output performance measures decrease ($\partial\gamma/\partial e < 0$ and $\partial\gamma_G/\partial e < 0$).

The intuition for $\partial\beta/\partial e > 0$ is straightforward and similar to findings from traditional agency models. Noisier output performance measures are more costly and therefore lead to assigning more weight to input performance measures. As the variance (e) of the output performance measure increases, the link between effort and performance weakens, which reduces employees' incentives to exert effort. We next describe our data and empirical tests.

III. EMPIRICAL MODEL

Sample Selection and Data Description

We obtain our data from the *4th Annual IndustryWeek Census of Manufacturers* (2000 Survey). The plants surveyed had two-digit SIC codes of 20–39, and survey respondents held titles such as manufacturing manager and plant manager, and reported on plant-level incentive features and other manufacturing practices at their facility. A total of 3,006 completed surveys were received (response rate 11 percent). We eliminate independently owned plants with fewer than 100 employees, because Heneman and Berkley (1999) suggest that smaller firms are more likely to rely on informal appraisal procedures.[7] Screening the resulting sample for missing information further reduced our sample to 1,780 observations. This sample reflects an industry distribution comparable to the Compustat population, and covers all two-digit manufacturing SIC codes with the most plants in Industrial and Commercial Machinery (Panel B of Table 1).[8] Panel C of Table 1 reports descriptive statistics for the sample plants, including sales, number of employees and plant age. The results indicate that the typical plant in our sample employs between 100 and 250 employees and has operated for over 20 years.

Translating Model Implications to Empirical Tests

Our model makes predictions concerning the incentive weights placed on alternative performance measures. Our empirical tests would ideally be based on the corresponding incentive weights in employees' compensation contracts. Unfortunately, the survey asked whether the plant had adopted various categories of input performance measures, as well as individual-based and group-based output performance measures, but did not collect the corresponding incentive weights. As a result, we design our empirical analysis to estimate the relative likelihood that plants use various categories of performance measures rather than to estimate the specific incentive weights they assign to these performance measures.[9]

To express our model's predictions in terms of the relative likelihood of plants using different categories of performance measures, we assume that whatever plant characteristics make it optimal to assign greater incentive weight to a particular performance category also increase the relative likelihood that the plant will find it cost-beneficial to employ such measures. For example, in environments in which employees have more extensive specific knowledge, Proposition 1 predicts that the firm will increase the incentive weights on output measures. Our assumption then implies that the greater is the employees' specific knowledge, the more likely the plant will be to compensate production employees on output performance measures.

Because our analysis addresses the *relative* likelihood that plants will adopt certain categories of performance measures, we must analyze measures that have been adopted by some, but not all plants. We assume that all plants employ a set of fundamental input performance measures, and further that all input performance measures are individual-based rather than group-based. The assumption that all plants base compensation to some extent

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[7] Our results are robust to including these observations, which comprise 5.6 percent of the total sample.

[8] Because respondents report at the plant level and we do not have firm identifiers, it is possible that multiple manufacturing plants from the same firm are included in the survey.

[9] For a similar approach see Garvey and Milbourn (2000) and Ittner and Larcker (2002).

TABLE 1
Descriptive Statistics[a]

Panel A: Distribution of Input-Output and Individual-Group Performance Measures

Incentive Base	Exclusively Input	Column Percentage	Input and Output	Column Percentage
Exclusively Individual	896	100.0%	249	28.2%
Individual and Group	0	0.0%	261	29.5%
Exclusively Group	0	0.0%	374	42.3%
Total Number	896		884	1,780
Row Percentage	50.3%		49.7%	100.0%

Panel B: Distribution of Sample across Industries

Two-Digit SIC Code	Industry	Count	Column Percentage
20	Food and kindred products	71	4.0%
21	Tobacco products	1	0.1%
22	Textile mill products	46	2.6%
23	Apparel and other finished products	13	0.7%
24	Lumber and wood products, except furniture	41	2.3%
25	Furniture and fixtures	34	1.9%
26	Paper and allied products	72	4.0%
27	Printing, publishing, and allied industries	40	2.3%
28	Chemicals and allied products	93	5.2%
29	Petroleum refining and related industries	9	0.5%
30	Rubber and miscellaneous plastics products	132	7.4%
31	Leather and leather products	6	0.3%
32	Stone, clay, glass, and concrete products	41	2.3%
33	Primary metal industries	106	6.0%
34	Fabricated metal products, except machinery	250	14.0%
35	Industrial and commercial machinery	307	17.3%
36	Electronic and other electrical equipment	250	14.0%
37	Transportation equipment	108	6.1%
38	Measuring, analyzing, and controlling instrument	107	6.0%
39	Miscellaneous manufacturing industries	53	3.0%
	Total	1,780	100.0%

Panel C: Sample Plant Descriptive Statistics

Plant Characteristic					
Plant Age	< 5 Years	5–10 Years	11–20 Years	> 20 Years	
	4.2%	8.3%	19.0%	68.5%	
Plant Full-Time Employees	< 100	100–249	250–499	500–999	> 1000
	9.5%	53.5%	23.0%	9.2%	4.8%
Sales of Parent Corporation ($Million)	No corporate parent	< $100	$100–499	$500–999	> $1,000
	18.9%	17.0%	20.3%	9.4%	34.4%

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[a] Sample covers all two-digit SIC Manufacturing Industries.

on individual inputs reflects the fact that plants typically track such input measures as each employee's time spent on the job, skill, and technical certifications, etc. However, employee compensation generally will not depend on the work records or skills of other employees; i.e., on group-based input measures. Therefore, we assume that all sample plants use individual-based input measures, but no plants use group-based input measures, and we then focus our empirical analysis on the relative likelihood that sample plants employ individual-based or group-based output performance measures. Given this perspective, we next translate the model's propositions into specific hypotheses.

Proposition 1 predicts that greater specific knowledge results in larger incentive weights on individual-based and group-based output measures. Translating this reasoning from incentive weights to the relative likelihood of incentive forms yields:

H1: The greater is the workers' specific knowledge in a manufacturing environment, the more likely plants are to use output performance measures.

Similarly, Proposition 2 predicts that the weight on output performance measures (both group- and individual-based output measures) increases as the value of information sharing among plant workers increases. Our second hypothesis translates this result concerning incentive weights to a prediction concerning the relative likelihood that the plant will employ output performance measures:

H2: The greater is the value of knowledge sharing in a manufacturing environment, the more likely a plant is to use output performance measures.

Next, Proposition 3 makes predictions concerning the relative influence of the value of knowledge sharing on individual-based output versus group-based output performance measures. Because we do not have data on specific incentive weights, we are unable to discern the relative importance of individual output versus group output performance measures in plants that use both types of output measures. Therefore, for this analysis we include only plants that use either exclusively individual-based output measures or exclusively group-based output measures. In turn, we formulate our hypotheses in terms consistent with this reduced sample. First, according to Proposition 3, increases in the level of workers' specific knowledge decrease (increase) the relative likelihood that these plants use exclusively group-based (individual) output performance measures. Second, plants that use output performance measures are more likely to use exclusively group-based (individual-based) output performance measures when the value of knowledge sharing among workers is relatively high (low).The preceding discussion results in the following hypothesis:

H3: As the extent of specific knowledge increases, the likelihood of using exclusively group-based (individual-based) output performance measures decreases (increases). Conversely, as the value of knowledge sharing increases, the likelihood of using exclusively group-based (individual-based) output performance measures increases (decreases).

Finally, in a similar manner we develop Hypothesis 4:

H4: As the noise in output performance measures increases, the likelihood of using output performance measures decreases.

Construction of Dependent Variable

As discussed above, the nature of our data requires that we estimate the likelihood of plants employing various categories of performance measures. Further, we assume that all plants employ a set of individual-based input performance measures and no plants use group-based input performance measures. Therefore, we focus our empirical tests on whether plants have adopted individual-based output performance measures and/or group-based output performance measures. Survey respondents indicated whether their plant implemented the following forms of compensation for production workers (respondents could select multiple responses): pay for knowledge, pay for skills, profit sharing, gain sharing, rewards for team performance, rewards for individual performance, other, and no incentives. We classify plants as using output measures ($OUTPUT = 1$) if they selected one or more of the following responses: gain sharing, rewards for team performance, rewards for individual performance.[10] We further classify these plants as using group-based output measures ($GROUP = 1$) if they indicated their plant used gain sharing or rewards for team performance, and we classify plants as using individual-based output measures if they indicated their plant used rewards for individual performance.

We treat responses of "pay for knowledge" and "pay for skills" as indicating that the plant uses input performance measures. For several reasons, we do not classify plants with a "profit-sharing plan" as using an output performance measure. First, profit-sharing plans generally create only a weak and deferred association between outcomes and production workers' compensation. In fact, Blinder (1990) and Blasi (1990) document that the majority of company-wide profit-sharing plans provide deferred compensation in order to achieve tax- and pension-planning advantages. Second, Welbourne and Gomez-Mejia (1995) argue that profit-sharing plans are less effective than gain-sharing plans in creating output-based incentives because production workers find it difficult to understand how their inputs translate into outputs. Finally, because profit-sharing contributions are usually proportional to employees' income, they generate strong incentives only for more highly paid middle and upper management.

Explanatory Variables

We next describe our proxies for specific knowledge and knowledge sharing and their relation to prior literature.

Specific Knowledge

Consistent with Jensen and Meckling (1992), we define specific knowledge as knowledge that is valuable to decision-making but very costly to transfer to managers or supervisors. Examples include knowledge of particular idiosyncrasies of customers, machinery, or operational processes.

Direct measures of the extent of specific knowledge in a particular setting are obviously difficult to obtain. To proxy for specific knowledge, we rely on the fact that a large body of empirical evidence concludes that the adoption of new manufacturing production technologies is generally associated with production workers who possess more extensive capabilities, skills and intelligence.[11] For example, using 1988 and 1993 U.S. Bureau of

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[10] An alternative approach, which complements our focus on the broader issue of the use of input versus output performance measures, is to analyze the use of each specific type of output performance measure, as in Ortega (2009).

[11] These new technologies, for instance, enable employees to deal with unforeseen contingencies, which will require them to have an understanding of the logic of the equipment's internal functioning and information on the equipment's status (Adler and Borys 1996).

Census Survey of Manufacturing Technology (SMT) data, Doms et al. (1997) examine how plant-level wages, occupational mix, workforce education, and productivity vary with adoption and use of new technologies. They find that plants that use a large number of new manufacturing technologies are more likely to employ more educated and technically skilled workers. Likewise, Capelli (1993) examines changes in skill requirements for production jobs in 93 manufacturing establishments between 1978 and 1986, a period in which major technological developments occurred (e.g., computer-aided design and manufacturing). His findings suggest that in this period the complexity of production workers' jobs also increased significantly. Similarly, Leigh and Gifford (1999) find that investments in production technology are positively associated with production workers' education level and hours of formal on-the-job training.

Based on the preceding evidence for the association between investments in new technology and employees' technical skills and capabilities, we use a survey item in which respondents categorize their plant as having either no implementation (*NEW TECH* = 1), some implementation (*NEW TECH* = 2), or extensive implementation (*NEW TECH* = 3) of new process equipment/technologies as a proxy for the extent of specific knowledge in each plant.[12]

Value of Knowledge Sharing

To proxy for the value of knowledge-sharing practices within each plant, we use an indicator that captures the percentage of the production workforce participating in empowered or self-directed teams (*EMPOWERMENT*).[13] In a review article on empowerment in the workplace, Spreitzer (2007, 4–5) identifies information sharing, skill/knowledge-based pay, and training as among the "specific practices that indicate a high involvement or self-managing system." Similarly, in an analysis of team dynamics, Seibert et al. (2004) use measures of information sharing, boundary setting, and team accountability to define the team climate that they relate to empowerment in organizations. Finally, Ichniowski et al. (1997) find high correlations among team practices (participation), information sharing (communication), and skills training in their sample of steel mill production processes. These studies provide general support for our use of *EMPOWERMENT* as a proxy for the value of knowledge sharing.[14]

Noise

We use the percentage of output that requires rework (i.e., 100 percent − first-pass quality yield rate) as a proxy for measurement noise, because we expect this measure to be positively associated with production process uncertainty. This uncertainty reflects the stochastic nature of scheduling, inventory, and maintenance processes, resulting in noisy measures of production workers' performance. To reduce the influence of outliers, we use the fractional rank of this measure. Thus, plants with greater values of this proxy represent plants with noisier output performance measures.

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[12] We check the validity of our *NEW TECH* measure by examining the correlation of this measure with various other measures in our sample data that correspond to results reported in the Bureau of Census SMT survey of Advanced Manufacturing Technologies. Consistent with *NEW TECH* being a reasonable measure of investment in advanced manufacturing technologies, we find significant positive correlation between *NEW TECH* and measures of Computer Aided Design (CAD), and Computer Integrated Manufacturing (CIM).

[13] We set *EMPOWERMENT* equal to 1, 2, 3, 4, 5, or 6 when respondents report that (1) 0, (2) 1–25, (3) 26–50, (4) 51–75, (5) 76–99, or (6) 100 percent, respectively, of the production workforce participates in empowered or self-directed teams.

[14] We check the validity of our *EMPOWERMENT* measure by examining the correlation of this measure with variables identified in the literature as being conducive to information sharing within organizations (e.g., Wruck and Jensen 1994; Cua et al. 2001). Consistent with *EMPOWERMENT* being a reasonable proxy for information sharing, we find significant positive correlation between *EMPOWERMENT* and measures of the implementation of cross-functional and employee problem-solving teams.

In addition to our hypothesized variables, we also include several control variables that are potentially correlated with our hypothesized variables. We control for the influence of non-production tasks on production workers by including a dummy variable that is equal to 1 when production workers interact with customers, and 0 otherwise (*CUST INT*). Because a plant's process technology likely affects both the types of skills required from manufacturing workers and the types of rewards used in the plant, we include several measures to control for production process characteristics. We control for a plant's capital intensity by including a logarithmic transformation of the dollar value of shipments per plant employee (*CAPITAL INTENSITY*). Further, following the classification by Hayes and Wheelwright (1984, 176–179), we include dummy variables that capture whether a plant's production process has a high mix of products (*MIX* = 1, 0 otherwise), produces a large volume of products (*VOLUME* = 1, 0 otherwise), is continuous (*CONTINUOUS* = 1, 0 otherwise) or is discrete (*DISCRETE* = 1, 0 otherwise).

In addition, we control for the extent to which plant production workers are represented by a union (*UNION*), because prior research suggests that unions prefer uniform pay to pay based on individual performance.[15] In particular, Verma (2005) finds that unionized workplaces are less likely to use individual incentive plans compared to nonunion workplaces, but are equally likely to adopt group incentive plans. Therefore, we expect unionization to be negatively associated with the use of individual-based output performance measures, but positively associated with the use of group-based output performance measures.

Further, because Heneman and Berkley (1999) suggest that a firm's size is associated with the use of formal appraisal systems, and Shah and Ward (2003) find that larger plants more often use innovative work practices, we control for plant size using a measure of the number of employees in each plant (*SIZE*).[16]

Prior research also suggests that organizations develop operational routines that change infrequently. In particular, Pil and MacDuffie (1996) suggest that the longer an organization has experience with these practices, the more difficult it is to replace older, inferior practices. Therefore, we include an indicator that captures the number of years that the plant has operated (*AGE*).[17] Finally, we include two-digit SIC indicator variables to control for industry effects.[18]

IV. EMPIRICAL RESULTS

Univariate Tests

Table 2 presents the results of univariate hypothesis tests using dichotomous measures of our main independent variables (subsequent tests use the full detail available for each variable). We classify plants that implemented new process equipment/technologies (*NEW TECH* > 1) as requiring a high degree of specific knowledge (*NEW TECH* = High, Low otherwise), plants that use empowered or self-directed teams (*EMPOWERMENT* > 1) as having a high value of knowledge sharing (*EMPOWERMENT* = High, Low otherwise),

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[15] Survey respondents categorized union representation at their plant as involving: (1) No workers (*UNION* = 1); (2) Some workers (*UNION* = 2); or (3) All workers (*UNION* = 3).

[16] Survey respondents categorize their plant as having (1) less than 100 (*SIZE* = 1); (2) 100–249 (*SIZE* = 2); (3) 250–499 (*SIZE* = 3); (4) 500–999 (*SIZE* = 4); or (5) 1,000 or more (*SIZE* = 5) employees.

[17] Survey respondents categorize the number of years since their plant's start-up as (1) less than 5 (*AGE* = 1); (2) 5–10 (*AGE* = 2); (3) 11–20 (*AGE* = 3); or (4) More than 20 (*AGE* = 4).

[18] Note that this implies that we drop two-digit SIC codes with less than two observations.

TABLE 2
Univariate Hypothesis Tests

		(1) Hypotheses 1, 2, and 4		(2) Hypothesis 3	
Measure	**High/Low**	***OUTPUT* = 0**	***OUTPUT* = 1**	***GROUP* = 0**	***GROUP* = 1**
NEW TECH	Low	155	85	26	37
(Specific Knowledge)		(64.6%)	(35.4%)	(41.3%)	(58.7%)
	High	741	799	223	337
		(48.1%)	(51.9%)	(39.8%)	(60.2%)
	Test Statistic	$\chi^2 = 22.52$	p-value = 0.00	$\chi^2 = 0.05$	p-value = 0.82
EMPOWERMENT	Low	321	170	72	71
(Knowledge Sharing)		(65.4%)	(34.6%)	(50.4%)	(49.6%)
	High	575	714	177	303
		(44.6%)	(55.4%)	(36.9%)	(63.1%)
	Test Statistic	$\chi^2 = 61.35$	p-value = 0.00	$\chi^2 = 8.34$	p-value = 0.00
NOISE	Low	361	425	113	177
		(45.9%)	(54.1%)	(39.0%)	(61.0%)
	High	535	459	136	197
		(53.8%)	(46.2%)	(40.8%)	(59.2%)
	Test Statistic	$\chi^2 = 10.94$	p-value = 0.00	$\chi^2 = 0.23$	p-value = 0.63
	n	896	884	249	374

Hypotheses 1, 2, and 4 are tested by contrasting plants that use output performance measures (*OUTPUT* = 1), with plants that do not use output performance measures (*OUTPUT* = 0). *OUTPUT* is equal to 1 if a plant offers monetary awards for gain sharing, group performance or individual performance, and 0 otherwise. Because we do not have data on incentive weights, we test H3 by contrasting plants that use exclusively group-based output measures (*GROUP* = 1), with plants that use exclusively individual-based output measures (*GROUP* = 0). *GROUP* is equal to 1 if a plant offers monetary awards for gain sharing or group performance, and is equal to 0 if a plant offers monetary awards for individual performance. For the purpose of this test we dichotomize our independent measures as follows: *NEW TECH* is Low if a plant has not implemented new process equipment/technologies, and High if a plant has some or extensive implementation of new process equipment/technologies; *EMPOWERMENT* is Low if respondents indicate that 0 percent of the production workforce participates in empowered or self-directed teams, and High if they indicated that 1–25 percent, 26–50 percent, 51–75 percent, 76–99 percent, or 100 percent of the production workforce participates in empowered or self-directed teams; *NOISE* is Low if the percentage of a plant's output that requires rework is below the median, and High otherwise.

and plants with an above the median rate of rework as having a high degree of measurement noise (*NOISE* = High, Low otherwise).

To test H1, H2, and H4 we examine the influence of our hypothesized variables on the proportion of plants that use output performance measures. The results of this analysis are presented in column (1) of Table 2. Consistent with our hypotheses we find that the proportion of plants that use output-based performance measures is positively associated with our proxies for specific knowledge (*NEW TECH*) and the value of knowledge sharing (*EMPOWERMENT*), and negatively associated with our proxy for measurement noise (*NOISE*) at $p < 0.01$ (one-tailed). Therefore, our univariate results support H1, H2, and H4.

As explained earlier, to test H3 we restrict our sample to plants that use either exclusively individual-based or exclusively group-based output performance measures, and examine whether our proxies for specific knowledge (*NEW TECH*) and the value of knowledge sharing (*EMPOWERMENT*) are associated with the proportion of plants that use exclusively group-based (as opposed to exclusively individual-based) output performance measures. Consistent with H3, column (2) of Table 2 shows that *EMPOWERMENT* is positively associated with the use of group-based performance measures. However, we find no significant association between *NEW TECH* and the use of group-based output performance measures. Therefore, our univariate tests provide mixed results for H3.

Multivariate Tests

Columns (1) and (2) of Table 3 present the results of a multivariate logit regression that tests H1, H2, and H4. Consistent with our hypotheses, the coefficients on *NEW TECH* (H1) and *EMPOWERMENT* (H2) in column (1) are positive and significant at $p < 0.01$ (one-tailed), and the coefficient on *NOISE* (H4) is negative and significant at $p < 0.05$. Moreover, in column (2) the magnitude of the marginal effects for our hypothesized variables suggests that they are of significant economic importance. In particular, the results in column (2) show that for a one unit increase in *NEW TECH* and *EMPOWERMENT* (e.g., increase from *NEW TECH* = 1 to *NEW TECH* = 2), the probability that a plant uses output measures increases by 7 percent and 6 percent, respectively.[19] In addition, plants in which production workers interact with customers (*CUST INT*), larger plants (*SIZE*), and younger plants (*AGE*) are more likely to use output performance measures. The remaining control variables are not significantly associated with the use of output performance measures.

In summary, the results reported in columns (1) and (2) of Table 3 are consistent with manufacturing plants using output performance measures to induce production workers to use and share their specific knowledge (*NEW TECH* and *EMPOWERMENT*), particularly when it is less costly to do so because measurement noise is low (*NOISE*).

Columns (3) and (4) of Table 3 present the results of testing H3. Consistent with H3, the coefficient on *NEW TECH* in column (3) is negative and significant at $p < 0.01$, and the coefficient on *EMPOWERMENT* is positive and significant at $p < 0.01$. Moreover, the results in column (4) show that our hypothesized variables are of significant economic importance. In particular, a one-unit increase in *EMPOWERMENT* (*NEW TECH*) increases (decreases) the probability that a plant uses group-based output measures by 9 percent. In addition, more capital intensive plants (*CAPITAL INTENSITY*), plants that produce a high volume of products (*VOLUME*), plants with a higher proportion of workers represented by unions (*UNION*), and younger plants (*AGE*) are more likely to use group-based output performance measures.

In summary, the results reported in columns (3) and (4) of Table 3 are consistent with group-based (as opposed to individual-based) output performance measures becoming more important when the value of knowledge sharing (*EMPOWERMENT*) is greater and when production workers possess less specific knowledge (*NEW TECH*).

V. ROBUSTNESS TESTS

Including Profit-Sharing as a Group-Based Output Measure

First, to check the sensitivity of our results to excluding profit-sharing plans as a group-based output measure, we repeat our multivariate analysis after recoding plants offering profit-sharing plans as using group-based output performance measures. The results reported in columns (1) and (2) of Table 4 confirm the earlier findings with respect to H1, H2, and

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[19] We use the STATA program margeff developed by Bartus (2005) to compute average marginal effects.

TABLE 3
Multivariate Logit Regressions

	OUTPUT = 1		*GROUP* = 1	
Measure	(1) **Coefficient (z-statistic)**	(2) **Avg. Marginal Effect (z-statistic)**	(3) **Coefficient (z-statistic)**	(4) **Avg. Marginal Effect (z-statistic)**
NEW TECH	0.32***	0.07***	−0.42***	−0.09***
	(3.75)	(3.86)	(−2.58)	(−2.68)
EMPOWERMENT	0.26***	0.06***	0.44***	0.09***
	(6.53)	(6.91)	(5.85)	(6.65)
NOISE	−0.41**	−0.09**	−0.26	−0.05
	(−2.19)	(−2.20)	(−0.75)	(−0.75)
CUST INT	0.27***	0.06***	0.15	0.03
	(3.14)	(3.20)	(0.94)	(0.94)
CAPITAL INTENSITY	−0.04	−0.01	0.16*	0.03*
	(−0.65)	(−0.65)	(1.61)	(1.63)
MIX	0.06	0.01	0.20	0.04
	(0.53)	(0.53)	(0.87)	(0.88)
VOLUME	−0.14	−0.03	0.37**	0.08**
	(−1.22)	(−1.22)	(1.80)	(1.86)
DISCRETE	−0.06	−0.01	0.18	0.04
	(−0.38)	(−0.38)	(0.60)	(0.61)
CONTINUOUS	−0.06	−0.01	0.00	0.00
	(−0.28)	(−0.28)	(0.01)	(0.01)
UNION	−0.07	−0.02	0.49***	0.10***
	(−1.03)	(−1.03)	(3.72)	(3.98)
SIZE	0.09*	0.02*	0.12	0.02
	(1.54)	(1.54)	(1.15)	(1.16)
AGE	−0.10*	−0.02*	−0.19**	−0.04**
	(−1.57)	(−1.57)	(−1.68)	(−1.70)
Number of Obs.	1,779		619	
Likelihood Ratio	159.79		93.87	

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*, **, *** Indicate one-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Hypotheses 1, 2, and 4 (column (1)) are tested by contrasting plants that use output performance measures (*OUTPUT* = 1), with plants that do not use output performance measures (*OUTPUT* = 0). *OUTPUT* = 1 if a plant offers monetary awards for gain sharing, group performance, or individual performance, 0 otherwise. We test H3 (column (3)) by contrasting plants that use exclusively group-based output measures (*GROUP* = 1), with plants that use exclusively individual-based output measures (*GROUP* = 0). *GROUP* = 1 if a plant offers monetary awards for gain sharing or group performance, and is equal to 0 if a plant offers monetary awards for individual performance.

Coefficients on two-digit SIC code indicators are not reported. Reported marginal effects are computed using the average of discrete or partial changes over all observations (Bartus 2005).

Variable Definitions:

NEW TECH = 1, 2, or 3 when plants have (1) no implementation, (2) some implementation, or (3) extensive implementation of New Process Equipment/Technologies, respectively;
EMPOWERMENT = 1, 2, 3, 4, 5, or 6 when respondents report that (1) 0 percent, (2) 1–25 percent, (3) 26–50 percent, (4) 51–75 percent, (5) 76–99 percent, or (6) 100 percent of the production workforce participates in empowered or self-directed teams, respectively;
NOISE = fractional rank of the percentage of output that requires rework;
CUST INT = 1 if a plant's production employees interact with customers, 0 otherwise;

(*continued on next page*)

TABLE 3 (continued)

CAPITAL INTENSITY = a logarithmic transformation of the dollar value of plant shipments per employee;
MIX = 1 if a plant produces a high mix of products, 0 otherwise;
VOLUME = 1 if a plant produces a high volume of products, 0 otherwise;
DISCRETE = 1 if a plant has a discrete production process, 0 otherwise;
CONTINUOUS = 1 if a plant has a continuous process, 0 otherwise;
UNION = 1, 2, or 3 when (1) no workers, (2) some workers, or (3) all plant production workers are represented by a union, respectively;
SIZE = 1, 2, 3, 4, or 5 when the plant has (1) less than 100, (2) 100–249, (3) 250–499, (4) 500–999, (5) 1,000 or more employees, respectively; and
AGE = 1, 2, 3 or 4 when it has been (1) less than 5; (2) 5–10; (3) 11–20; or (4) more than 20 years since plant start-up, respectively.

H3 but not H4. In particular, in column (1) the coefficients on *NEW TECH* (H1) and *EMPOWERMENT* (H2) remain positive and significant at $p < 0.01$. In addition, in column (2) the coefficient on *NEW TECH* remains negative and significant at $p < 0.05$, and the coefficient on *EMPOWERMENT* remains positive and significant at $p < 0.01$ (H3). However, although the estimated coefficient on measurement noise (*NOISE*) in column (1) remains negative, it is not statistically significant (H4). Therefore, our results with respect to how specific knowledge and the value of knowledge sharing influence performance measure choice are robust to the inclusion of profit-sharing plans as a group-based output performance measure, but our results for measurement noise are not.

Alternative Contrast for Testing Hypothesis 3

The earlier test of H3 considered only plants that use either exclusively individual-based or exclusively group-based output performance measures, thereby excluding 261 plants that use both types of output measures. For this second robustness check, we include these 261 plants by contrasting plants using either exclusively group-based or group- and individual-based output measures with plants using exclusively individual-based output measures. The results in column (3) of Table 4 show that consistent with the earlier findings, the coefficient on *NEW TECH* remains negative and significant at $p < 0.05$, and the coefficient on *EMPOWERMENT* remains positive and significant at $p < 0.01$. Therefore, the results with respect to H3 are robust to including plants that use both individual- and group-based output performance measures.

Using Labor Training as an Alternative Proxy for Specific Knowledge

The survey collected data on the average annual hours of formal training received by each plant employee.[20] Because such training is likely to increase production workers' specific knowledge, a measure of the extent of relevant training could be an alternative proxy for specific knowledge. However, Bureau of Labor Statistics (1996) data suggests that much of manufacturing workers' formal training involves issues largely unrelated to specific knowledge (e.g., occupational safety, communications, and quality training). Because we do not have data on the specific nature of the training received by production workers, we use the number of training hours multiplied by the percentage of goods sold attributable to labor costs (*LABOR TRAINING*) as an alternative proxy for specific knowledge. To the extent that the percentage of costs of goods sold attributable to labor costs reflects production workers' skills, education, and other characteristics related to specific

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[20] Categories for hours of annual training were (1) less than 8; (2) 8–20; (3) 21–40; and (4) more than 40 hours.

TABLE 4
Robustness Tests

	Alternative Dependent Measure			Alternative Independent Measure	
Measure	**(1) Hypotheses 1, 2, and 4** ***OUTPUT 2* = 1**	**(2) Hypothesis 3** ***GROUP 2* = 1**	**(3) Hypothesis 3** ***GROUP 3* = 1**	**(4) Hypotheses 1, 2, and 4** ***OUTPUT* = 1**	**(5) Hypothesis 3** ***GROUP* = 1**
NEW TECH	0.30*** (3.23)	−0.39** (−2.30)	−0.30** (−2.10)		
LABOR TRAINING				0.20** (1.67)	−0.53** (−2.20)
EMPOWERMENT	0.22*** (4.87)	0.37*** (4.07)	0.45*** (6.38)	0.29*** (7.00)	0.44*** (5.63)
NOISE	−0.15 (−0.73)	0.06 (0.17)	−0.19 (−0.63)	−0.54*** (−2.82)	0.01 (0.02)
CUST INT	0.36*** (3.69)	0.09 (0.56)	0.32*** (2.35)	0.25*** (2.89)	0.07 (0.42)
CAPITAL INTENSITY	−0.04 (−0.57)	0.10 (0.94)	0.22*** (2.40)	−0.03 (−0.48)	0.14* (1.35)
MIX	0.08 (0.59)	−0.04 (−0.14)	0.17 (0.84)	0.11 (0.90)	0.14 (0.59)
VOLUME	−0.26** (−2.07)	0.12 (0.56)	0.33** (1.77)	−0.07 (−0.62)	0.43** (2.02)
DISCRETE	−0.05 (−0.30)	0.17 (0.53)	0.07 (0.26)	−0.07 (−0.41)	0.04 (0.12)
CONTINUOUS	−0.25 (−1.19)	−0.03 (−0.07)	0.01 (0.04)	−0.03 (−0.14)	0.00 (0.00)
UNION	−0.40*** (−5.95)	−0.06 (−0.47)	0.38*** (3.22)	−0.05 (−0.73)	0.48*** (3.52)
SIZE	0.06 (0.94)	0.11 (0.97)	0.18** (1.90)	0.12** (2.09)	0.05 (0.51)
AGE	−0.05 (−0.69)	−0.04 (−0.29)	−0.14* (−1.39)	−0.11** (−1.77)	−0.17* (−1.46)
Number of obs.	1,779	834	879	1,670	574
Likelihood Ratio	162.98	45.62	106.97	143.08	88.23

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*, **, *** Indicate one-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Columns (1), (2), and (3) present the results of robustness tests in which we alter our dependent measure. In columns (1) and (2) we include profit-sharing plans as group-based output performance measures. *OUTPUT 2* is equal to 1 if a plant offers monetary awards for profit sharing, gain sharing, group performance, or individual performance, and 0 otherwise. *GROUP 2* is equal to 1 if a plant offers monetary awards for profit sharing, gain sharing, or group performance, and is equal to 0 if a plant offers monetary awards for individual performance. In column (3) we contrast plants using exclusively group-based or using both group-based and individual-based output measures (*GROUP 3* = 1), with plants using exclusively individual-based output measures (*GROUP 3* = 0). *GROUP 3* is equal to 1 if a plant offers monetary awards for gain sharing or group performance, and is equal to 0 if a plant offers monetary awards for individual performance, but not for gain sharing or group performance. Columns (4) and (5) present the results of robustness tests in which we use *LABOR TRAINING* as an alternative proxy for production workers' specific knowledge. *LABOR TRAINING* is the average annual hours of formal training received by each plant employee multiplied by the percentage of costs of goods sold attributable to labor costs.

(*continued on next page*)

TABLE 4 (continued)

See Table 3 for remaining variable definitions.
Coefficients on two-digit SIC code indicators are not reported.
Table reports coefficient estimates, and their corresponding z-statistics (z-statistics are reported in parentheses).

knowledge, we expect total training hours weighted by this percentage to be a reasonable proxy for specific knowledge.[21]

Columns (4) and (5) of Table 4 show the results of using *LABOR TRAINING* as a proxy for specific knowledge. Consistent with our earlier findings, the coefficient on *LABOR TRAINING* is positive and significant at $p < 0.05$ when we test H2 (column (4)), and negative and significant at $p < 0.05$ when we test H3 (column (5)). In addition, also all our other predictions remain supported. In particular, the coefficient on *EMPOWERMENT* remains positive and significant when we test H2 (column (4)) and H3 (column (5)) at $p < 0.01$, and the coefficient on *NOISE* remains negative and significant at $p < 0.01$ when we test H4 (column (4)). Therefore, our results are robust to using training for skilled labor as an alternative proxy for specific knowledge.

VI. DISCUSSION AND CONCLUSIONS

We develop and test a parsimonious model relating specific knowledge, the value of knowledge sharing, and measurement noise to manufacturing plants' performance measure choices. Our empirical results provide general support for the prediction that manufacturing plants are more likely to use output performance measures when production workers have greater specific knowledge (proxied by the implementation of new production equipment/technologies) or when it is more valuable to induce production workers to share their specific knowledge (proxied by the percentage of the production workforce participating in empowered or self-directed teams). In addition, consistent with traditional agency models, we find empirical support for our model's prediction that the use of output performance measures is negatively associated with measurement noise (proxied by the percentage of output that requires rework). Moreover, consistent with our model's prediction, we find that when plants use output performance measures, they are more likely to use exclusively group-based (as opposed to exclusively individual-based) output measures when the value of knowledge sharing is greater and when, production workers possess less specific knowledge.

Our study makes several contributions to the incentive design literature. First, previous empirical studies have predominantly focused on the informativeness of performance measures (e.g., Murphy 2000; Bushman et al. 1995). Our study complements such previous empirical work by showing that, under certain conditions, even when input performance measures are less noisy than output performance measures, principals reward agents based on their output when agents possess sufficient specific knowledge. Second, by modeling specific knowledge and the sharing of this knowledge as related but distinct concepts, our analysis complements and extends prior research that focuses on the influence of specific knowledge on incentive design (e.g., Raith 2008; Ortega 2009). Our results demonstrate how specific knowledge and knowledge sharing both influence firms' choices between individual-based and group-based incentives.

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[21] If we instead use total training hours unadjusted for labor costs, the coefficient on total training hours is positive and significant for H1, but insignificant for H3 (all other findings remain supported). We conjecture that these weaker results for H3 are the result of a significant portion of total training for manufacturing workers being related less to employees' specific knowledge and more to the value of employee knowledge sharing (e.g., communications and quality training).

Finally, our sample of 1,780 plants spans all of the two-digit manufacturing SIC codes, giving our results greater generality than previous research that relies on either relatively small samples (e.g., Bushman et al. 1995) or results from a single industry (e.g., Ichniowski et al. 1997).

Although we performed several robustness tests to corroborate our findings, the use of archival data collected by a third party makes our results subject to several important limitations. First, the survey questions were not designed for our specific hypotheses, and therefore our proxies can only imperfectly capture the underlying constructs, particularly the extent of specific knowledge and the value of knowledge sharing. Second, data on the implementation of manufacturing practices is self-reported, which could potentially bias our findings.

APPENDIX

PROOFS

Group Task Information Structure

Consider the following general form of the information structure, where the prior probability of high versus low productivity equals: $p(\tau_G = 1) = p(\tau_G = -1) = 1/2$:

(s_{iG}, s_{jG})	$p(s_{iG}, s_{jG} \vert \tau_G = 1)$	$p(s_{iG}, s_{jG} \vert \tau_G = -1)$
(1, 1)	$\frac{(1 + k + \rho)}{4}$	$\frac{(1 - k - \rho)}{4}$
(1, −1)	$\frac{(1 + k)}{4}$	$\frac{(1 - k)}{4}$
(−1, 1)	$\frac{(1 - k)}{4}$	$\frac{(1 + k)}{4}$
(−1, −1)	$\frac{(1 - k - \rho)}{4}$	$\frac{(1 + k + \rho)}{4}$

The parameter ρ captures the value of knowledge sharing between workers and $\rho \in [0, 1 - k]$. The information structure can be expressed as: $p(s_{iG}, s_{jG}|\tau_G) = \frac{1}{4}\left[1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG})\tau_G\rho\right]$. Based on the information structure of $p(s_{iG}, s_{jG}|\tau_G)$ defined above, conditional probability of $p(\tau_G| s_{iG}, s_{jG})$ can be expressed as:

$$p(\tau_G|s_{iG}, s_{jG}) = \frac{\frac{1}{2}p(s_{iG}, s_{jG}|\tau_G)}{\frac{1}{2}p(s_{iG}, s_{jG}|\tau_G = 1) + \frac{1}{2}p(s_{iG}, s_{jG}|\tau_G = -1)}$$

$$= \frac{1}{2}\left[(1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG})\tau_G\rho\right]$$

with:

	$s_{iG} = s_{jG} = 1$	$s_{iG} = -1, s_{jG} = 1$	$s_{iG} = 1, s_{jG} = -1$	$s_{iG} = s_{jG} = -1$
$p(\tau_G = 1 \mid s_{iG}, s_{jG})$	$\frac{(1+k+\rho)}{2}$	$\frac{(1+k)}{2}$	$\frac{(1-k)}{2}$	$\frac{(1-k-\rho)}{2}$
$p(\tau_G = -1 \mid s_{iG}, s_{jG})$	$\frac{(1-k-\rho)}{2}$	$\frac{(1-k)}{2}$	$\frac{(1+k)}{2}$	$\frac{(1+k+\rho)}{2}$

Thus, when workers share their knowledge and receive conflicting signals (e.g., $s_{iG} = -1$, $s_{jG} = 1$) the corresponding posterior probability on the productivity parameter τ_G converges to the prior. As the value of information sharing ρ approaches its maximum value of $(1 - k)$, workers can achieve perfect information on productivity by pooling their knowledge; i.e., when $\rho \rightarrow (1 - k)$ we have $p(\tau_G = 1 \mid s_{iG} = 1, s_{jG} = 1) \rightarrow 1$ and $p(\tau_G = -1 \mid s_{iG} = 1, s_{jG} = 1) \rightarrow 0$.

Expected Value of Productivity Parameter Condition on Pooled Signals (Equation (5))

Denote $\hat{\theta}_G(s_{iG}, s_{jG})$ as the expected value of θ_G (the productivity parameter of the group task) conditional on workers' pooled private signals (s_{ig}, s_{jG}), where $s_{iG}, s_{jG}, \tau_G \in \{1,-1\}$. The information structure defined above implies the following expected group productivity parameter:

$$\begin{aligned}
\hat{\theta}_{iG} &= E(\theta_G \mid s_i, s_j) = \sum_{\tau_G} \bar{\theta}(1 + \tau_G)p(\tau_G \mid s_{iG}, s_{jG}) \\
&= \sum_{\tau_G} \bar{\theta}(1 + \tau_G)\frac{1}{2}\left[(1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG})\tau_G \rho\right]; \text{ where } \tau_G = +1 \textit{ or } -1 \\
&= \bar{\theta}\left[1 + s_{iG}k + \frac{(s_{iG} + s_{jG})}{2}\rho\right].
\end{aligned}$$

Derivation of Principal's Expected Profit Function

Substituting into the *ex ante* expected profit function Equation (7) in the main text for the expected performance measure from Equations (2) and (3), the principal's expected profit function can be expressed as:

$$\begin{aligned}
E(\pi) = {} & E_I((1 - \gamma(1 - e)) \times [\theta_i a_i + \theta_j a_j]) + E_I[(1 - 2\gamma_G(1 - e)) \\
& \times (\theta_G(a_{iG} + a_{jG}))] - E_I[\beta(A_i + A_j)] - e(\gamma + \gamma_G); \\
\text{where } I = {} & \{s_i, s_j, s_{iG}, s_{jG}, \tau_i, \tau_i, \tau_G\}. \qquad \text{(A1)}
\end{aligned}$$

Next, substitute the expected productivities (5) back into (6) for the optimal actions and using the resulting expressions into (A1) and evaluate the expected value of (A1) over the signals $(s_i, s_j, s_{iG}, s_{jG}, \tau_i, \tau_j, \tau_G)$. The expected value of the resulting expression reflects 128 permutations (the four signals s_i, s_j, s_{iG}, and s_{jG} each have two possible values, as do the three production technology parameters, τ_i, τ_j, and τ_G) with corresponding probability:

$$\frac{(1 + k\tau_i s_i) \times (1 + k\tau_j s_j) \times [(1 + s_{iG}\tau_G k) + \frac{1}{2}(s_{iG} + s_{jG}) \times \tau_G \rho]}{128}.$$

The above leads to the following expression for the principal's *ex ante* expected profit:

$$E\pi = \begin{pmatrix} \left(\frac{(1-e)}{d}\right)[\gamma(1+k^2)(1-(1-e)\gamma)]\bar{\theta}^2 + \left(\frac{(1-e)}{2d}\right)\gamma_G(1-2(1-e)\gamma_G) \\ (2+(1-e)\gamma_G(2+(\rho+k)^2))\bar{\theta}^2 - \beta\left(\frac{2\beta + [(1-e)(2\gamma+3\gamma_G)-2]\bar{\theta}}{d}\right) \\ -e(\gamma+\gamma_G) \end{pmatrix}.$$

By taking the first order derivative of $E\pi$ with respect to the incentive weights: (β, γ, and γ_G) we arrive at the optimal incentive weights:

$$\beta = \frac{(1-e)\bar{\theta}^2[(2+(\rho+k)^2)(1+k^2)] + 2de[7+(3k^2+2(\rho+k)^2)]}{2(1-e)[(7k^2+4(\rho+k)^2(1+2k^2))-1]\bar{\theta}}$$

$$\gamma = \frac{(1-e)\bar{\theta}^2[(7k^2+(\rho+k)^2(3+8k^2))-3] - de(13+8(\rho+k)^2)}{2(1-e)^2[(7k^2+4(\rho+k)^2(1+2k^2))-1]\bar{\theta}^2}$$

$$\gamma_G = \frac{(1-e)\bar{\theta}^2[(k^2+(\rho+k)^2(1+2k^2))-1] - de(5+4k^2)}{(1-e)^2[(7k^2+4(\rho+k)^2(1+2k^2))-1]\bar{\theta}^2}. \tag{A2}$$

A necessary condition to ensure interior solution for positive incentive weight requires:

$$(1-e)\bar{\theta}^2[(k^2+(\rho+k)^2(1+2k^2))-1] > de(5+4(\rho+k)^2). \tag{A3}$$

Assumption (A3) ensures the numerator of γ_G is positive. It also implies $(k^2 + (\rho + k)^2(1 + 2k^2)) - 1 > 0$; which in turn implies $[(7k^2 + 4(\rho + k)^2(1 + 2k^2)) - 1] > 0$, hence $\beta > 0$. Similarly, one can show (A3) implies $\gamma > 0$.

Proof of Proposition 1: To examine the effects of specific knowledge k on agents' incentive weights, we take the partial from the expressions in (A2) for the optimal incentive weights, we have: $\frac{\partial\beta}{\partial k} = < 0$; similar calculations yield $\frac{\partial\gamma}{\partial k} > 0$ and $\frac{\partial\gamma_G}{\partial k} > 0$.

Proof of Proposition 2: From the expressions in (A2) for the optimal incentive weights, by taking the partial derivative of the incentive weights with respect to the parameter for the value of knowledge sharing ρ; Assumption (A3) ensures we have: $\frac{\partial\beta}{\partial\rho} < 0$, $\frac{\partial\gamma}{\partial\rho} > 0$ and $\frac{\partial\gamma_G}{\partial\rho} > 0$.

Proof of Proposition 3: Given that $k \in [0,1]$, we have $k^2 \leq 1$. Using the expressions for optimal incentive weights in (A2), it follows that for $k > 1/2$ we have $\frac{\partial(\gamma_G - \gamma)}{\partial \rho} = > 0$. A similar approach yields: $\frac{\partial(\gamma_G - \gamma)}{\partial k} = < 0$.

Proof of Proposition 4: From the optimal incentive weight expressions in (A2), and Assumption (A3) we get $\frac{\partial \beta}{\partial e} = > 0$. Similar analysis yields:

$$\frac{\partial \gamma}{\partial e} = \frac{\bar{\theta}^2(1 - e)[(7k^2 + (\rho + k)^2(3 + 8k^2)) - 3] - d(1 + e)(8(\rho + k)^2 + 13)}{(1 - e)^3[(7k^2 + 4(\rho + k)^2(1 + 2k^2)) - 1]\bar{\theta}^2}. \tag{A4}$$

The denominator of (A4) is positive because of (A3). Hence, the sign of $\partial\gamma/\partial e$ is determined by the sign of the numerator. Note $(\rho + k) \leq 1$ and $k \leq 1$. Hence the numerator of (A4) can be expressed as:

$$\bar{\theta}^2(1 - e)[(7k^2 + (\rho + k)^2(3 + 8k^2)) - 3] - de(8(\rho + k)^2 + 13) \\ - d(8(\rho + k)^2 + 13).$$

It can be shown that for $d \geq \bar{\theta}^2$ we have:

$$d(8(\rho + k)^2 + 13) \geq \bar{\theta}^2(1 - e)[(7k^2 + (\rho + k)^2(3 + 8k^2)) - 3].$$

Thus, $\frac{\partial \gamma}{\partial e} \leq 0$; similarly, one can show that $\frac{\partial \gamma_G}{\partial e} \leq 0$.

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Market Reaction Surrounding the Filing of Periodic SEC Reports

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ABSTRACT: Using data from the EDGAR era, we find a significant market reaction surrounding quarterly periodic reports only when their filing coincides with the first public disclosure of earnings, although that for 10-K reports is not subsumed by earnings releases. However, after eliminating incidence of concurrent earnings releases, the 10-K market reaction is restricted to a quarter of the reports that are filed around calendar quarter-ends. The calendar quarter-end price and volume effects are unrelated to the filing of periodic reports and are not explained by self-selection. However, while the quarter-end volume reaction is indistinguishable between filers and non-filers, we find an incremental price reaction to 10-K filings at calendar quarter-ends in recent times. We provide evidence that the calendar-time effect is partially due to an intra-industry information transfer that is a function of the incidence of 10-K reports at quarter-ends. Finally, equity analyst reactions are muted around periodic filings, with no evidence that they contribute to quarter-end information transfer.

Keywords: *periodic SEC reports; market reaction; concurrent earnings release; calendar quarter-end effect.*

Data Availability: *Data used in this study are available from public sources identified in the article.*

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I. INTRODUCTION

This study re-examines the market reaction (price and volume) surrounding the filing of periodic SEC reports in the EDGAR era. Academic research using data from the pre-EDGAR period yields mixed results for market reaction surrounding the filing of periodic SEC reports.[1] However, recent studies employing EDGAR-era data find statistically significant stock price reactions around the filing of periodic SEC reports.[2] Griffin (2003) is the first study to show significant stock price reactions surrounding both 10-K *and* 10-Q filings in a large-sample setting in the EDGAR era. We provide alternative explanations to such findings by focusing on the phenomenon in which a segment of the publicly traded companies files their periodic SEC reports on the same day earnings information is released to the market for the first time.[3] Given the extensive literature on the economic and statistical significance of market reaction to earnings announcements (Kothari 2001), we consider the effects of concurrent earnings releases on the measurement of market reaction surrounding the filing of periodic reports.

In addition, while EDGAR provides more instantaneous access to information in periodic SEC filings, extant research suggests that earnings announcements may be increasingly pre-empting financial statement disclosures in mandatory SEC filings. Limited academic research suggests that increasing numbers of firms are providing detailed income statement and balance sheet information in their earnings press releases (Francis et al. 2002; D'Souza et al. 2008). The extent to which earning announcements may act as a substitute for the SEC filings provides an additional impetus for re-examining the extant evidence on the market reaction surrounding periodic SEC filings.[4]

Our analysis is based on a sample of over 240,000 periodic SEC filings over the calendar years 1996 to 2006. Our descriptive evidence indicates that in roughly 22.7 (16.4) percent of quarterly (annual) SEC reports, the filing date coincides with the first release of earnings information.[5] Our multiple regression results indicate that quarterly reports are associated with significant stock price reactions only when firms concurrently issue an earnings press release, or when quarterly reports are the first public source of earnings information (Easton and Zmijewski 1993). The stock price movements surrounding the 10-Q and 10-QSB filing dates that follow an earnings announcement are significantly lower than that of the residual category of non-event-days. However, even after we control for concurrent earnings releases, we find a statistically significant investor reaction around 10-K filings, although its economic significance is lower. Our analysis using trading volume around the filing of annual and quarterly reports is consistent with that based on stock price changes.

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[1] Using data from the pre-EDGAR era, early research found mixed evidence for the information content of 10-K filings and annual reports (Foster and Vickrey 1978; Stice 1991; Swaminathan 1991; Foster et al. 1983; Cready and Mynatt 1991). Using an extensive database of exchange listed (1966–1985) and NASDAQ (1973–1985) firms, Easton and Zmijewski (1993) find no abnormal stock price reaction to either 10-K or 10-Q filings except in limited circumstances.

[2] Using a smaller sample, Asthana and Balsam (2001) find that the electronic filing requirements under EDGAR have resulted in a more timely dissemination of information through 10-K filings. Qi et al. (2000) appear to focus on firms that were part of the EDGAR phase-in period 1993–1995 before EDGAR became mandatory on May 6, 1996 (Lexis-Nexis, EDGAR Dissemination Service Subscriber Documentation, Release 4.1, April 1995).

[3] We distinguish "earnings announcement" from "earnings release." By "earnings announcement," we refer to corporate disclosure of annual or interim earnings to the public through a press release. We define "earnings release" as either a press release or a periodic SEC filing through which earnings information is released to the public for the first time.

[4] The increased market focus on earnings release is consistent with the practitioner view that "[b]y the time that a 10-Q is filed, the analyst and investor community is already focusing on what to expect for the next earnings release instead of spending 'quality time' with the filing just completed" (http://www.accountingobserver.com/commentary/letters/2002/sec-a). Afterman (1995, 12) suggests that "[b]ecause the market will already have digested all important accounting information by the time financial statements are issued, the impact of such statements—unless they contain surprises—will be immaterial." Also see Union Planters Corporation's comment letter to the SEC dated May 17, 2002.

Due to the mandatory filing deadlines, nearly 26 percent of all 10-K filings are made within five trading days surrounding calendar quarter-ends in our sample period. We conduct a battery of tests to examine whether the market reaction surrounding 10-K reports may be due to filings that cluster in a particular calendar-time. First, we examine whether our findings may be linked to the turn-of-the-quarter phenomenon documented in the finance research. Extant finance research finds that increased trading volume and price reactions around calendar quarter-ends are consistent with the incentives of mutual fund managers and other institutional investors to window dress and "lean for the tape" (Lakonishok et al. 1991; Musto 1999; Carhart et al. 2002; Morey and O'Neal 2006; Meier and Schaumburg 2006). When we condition our analysis on calendar-time clustering, both price and volume analyses indicate that statistically significant market reactions are present only for the quarter of the 10-K filings that are made around calendar quarter-ends. In addition, we document price and volume reactions at calendar quarter-ends unrelated to filing of periodic reports. However, while volume reactions for calendar quarter-ends with a 10-K filing are statistically indistinguishable from those without a filing, there is some evidence of an incremental price reaction to 10-K filing in recent times.

Second, we consider the possibility that the higher market reaction surrounding calendar quarter-end 10-K filings could be a reflection of the characteristics of the firms that choose to file at that time. Contrary to this conjecture, we find that the significant calendar quarter-end effect in stock price and trading volume persists even after we control for possible self-selection in firms' choice of 10-K filing date. In addition, consistent with a calendar-time effect, when firms uncharacteristically deviate from non-calendar quarter-end filing, they experience a significantly higher market reaction. Collectively, our analysis suggests that the increased market reaction around calendar quarter-end 10-K filings does not appear to be driven by self-selection in the timing of 10-K reports.

Finally, we surmise a heightened market focus during times in which information related to a large number of companies is clustered in calendar-time (see Hirshleifer et al. [2008] for a contrary argument). Such a focus could translate into information transfers from firms filing periodic reports around calendar quarter-ends to non-filing firms in the same industry, thereby generating a calendar-time, market-level effect. After controlling for the calendar-quarter effect, we find a positive association between the market reaction of non-filing firms at the end of March and the incidence of periodic reports filed at that time by other firms in the industry, consistent with intra-industry information transfer.

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[5] Two observations are in order. First, Amir and Livnat (2005) examine the characteristics of firms that consistently issue preliminary earnings announcements versus those that consistently file only the SEC reports without a preliminary earnings announcement. Second, some firms that do not issue an earnings press release prior to filing the SEC report do so at the time of the filing. We recognize that the joint information effects of concurrent earnings press releases and SEC filings could be different from those periodic SEC filings that are not accompanied by an earnings press release. While interesting in their own rights, we do not focus on the marginal incremental effects of issuing a press release given it is not germane to our main research question. In addition, as we discuss below, Compustat does not offer enough information to identify cases in which an SEC filing occurs concurrently with the issuance of an earning press release, subsequent to the year 1999 (see Amir and Livnat 2005).

In addition to overall market reactions, we also examine the reactions of an important set of market participants to periodic SEC filings, namely, equity analysts. After controlling for concurrent earnings releases, there is neither evidence of significant forecast revisions surrounding periodic SEC reports that follow an earnings announcement, nor is there an analyst reaction clustered around calendar quarter-ends. The lack of a calendar quarter-end effect in analyst forecast revisions surrounding 10-K reports suggests that the information transfer must be due to the activities of other market participants.

Taken together, our study provides large-sample evidence that significant market reaction surrounding 10-Q/QSB/KSB reports is limited to filings that release earnings information for the first time, while significant reaction also is obtained in 10-K reports when they are filed around calendar quarter-ends. One key takeaway from our study is with regard to the effects of disclosure endogeneity on market reaction tests surrounding corporate information releases. Our research adds to the recent work by Ecker et al. (2006), who suggest that confluence of corporate disclosures and news events are both frequent and non-random, and have important implications for understanding the economic and statistical significance of market reactions to firm-specific announcements.

As a last step, we discuss the implications of our findings for future research. We also illustrate disclosure endogeneity in small- and large-sample contexts. We suggest that circumstances in which researchers expect significant new information in periodic reports may also be the same circumstances in which firms may have incentives to provide complementary disclosures through a different medium. Using a small-sample context, we show that when firms report a lower earnings number in their periodic filings compared to the figure reported in the earnings press releases, they almost invariably provide preemptive or concurrent press releases highlighting the downward revision. While there is evidence of price reaction to such downward revisions (Hollie et al. 2005, 2006), future research can extend the literature by analyzing which of the competing disclosure media draw investors' attention to the adverse information. Research along these lines could provide valuable evidence on the managerial choice regarding the salience of disclosure channels (Bamber and Cheon 1998; Stice 1991).

In a large-sample context, we examine the case of delayed periodic filings in which companies face significant economic events (Alford et al. 1994), are subject to greater market scrutiny in general, and consequently have incentives to provide disclosures to mitigate information risk. To examine the market reaction surrounding delayed filings, we use Callen et al.'s (2006) variance decomposition approach and estimate the discount rate, operating cash flow, and accrual news as reflected in the volatility of unexpected returns around 10-K filings. After controlling for delayed disclosures, only for discount rate do we find a significant incremental news effect surrounding the 10-K filing date when compared to non-event periods. While we find statistically and economically significant cash flow and accruals news effects surrounding delayed filings, the estimated effects are indistinguishable from the non-event period. This finding is consistent with our arguments that firms planning to delay their filings will have incentives to disclose more generally and are subject to greater market scrutiny.

Our sample selection procedure is explained and descriptive statistics on concurrent earnings releases are provided in Section II. Section III examines the incremental market reaction surrounding periodic SEC reports after controlling for concurrent earnings releases, whereas Section IV documents and presents different explanations for the calendar-quarter effect in the market reaction surrounding 10-K reports. In Section V, we examine analyst reactions to earnings releases versus periodic SEC filings. Section VI provides suggestions for future research within the context of disclosure endogeneity documented in this study. Concluding remarks are provided in the final section.

II. SAMPLE SELECTION AND DESCRIPTIVE EVIDENCE ON CONCURRENT EARNINGS RELEASES

Sample Selection

The population of all annual and interim filing dates during the calendar years 1996 (the first year of EDGAR) through 2006 was downloaded from the 10kWIZARD database. Specifically, we focus on forms 10-K (including 10-K405 filed by delinquent registrants), 10-KSB, 10-Q, and 10-QSB. Given that our study examines the market reaction surrounding the filings of routine periodic reports, we exclude both amended filings (e.g., 10-K/A) and notifications of inability to timely file annual and quarterly reports (NT 10-K and NT 10-Q; Alford et al. 1994). This provides a sample of almost 515,000 annual and quarterly periodic filing dates during the period 1996–2006, with more than 134,000 annual report filing dates, and nearly 381,000 quarterly report filing dates.

We then combine our 10kWIZARD database with the Compustat and CRSP databases to identify companies with earnings announcement dates and stock returns. This process eliminates the filing dates corresponding to non-publicly traded subsidiaries, trusts, and other SEC filers.[6] We further exclude observations in which the filing dates or the earnings announcement dates are before the end of the fiscal quarter, indicating possible coding errors in the databases (242 observations). The sample for our market reaction tests consists of 243,561 annual and quarterly periodic filing dates over an 11-year period. The sample represents 10,240 firms with 1,805 SB filers, and 9,583 non-SB firms, with 1,148 firms shifting from one category to the other during the sample period.

Concurrent Earnings Releases

In this section we provide evidence regarding both calendar-time and event-time clustering of earnings releases and periodic SEC reports.[7] Figure 1, Panels A and B, provide the calendar-time distribution of quarterly and annual filings, respectively, during our sample period. We count the number of filings for each calendar day during each year in our sample period and average the daily counts over the 11-year period. The inter-temporal average daily counts are plotted in Figure 1, Panels A and B, with the calendar dates indicated on the x-axis (e.g., the number "6.29" corresponds to June 29). Between 7 and 8 percent of all interim filings during a calendar year occur on May 15, August 14, and November 14, collectively accounting for 22.5 percent of all interim filings. In addition, the six days ending on the 45th day after the end of the interim quarters of calendar-year firms account for almost 52 percent of all 10-Q/QSB filing dates in our sample (compared to 43 percent in Griffin [2003]).

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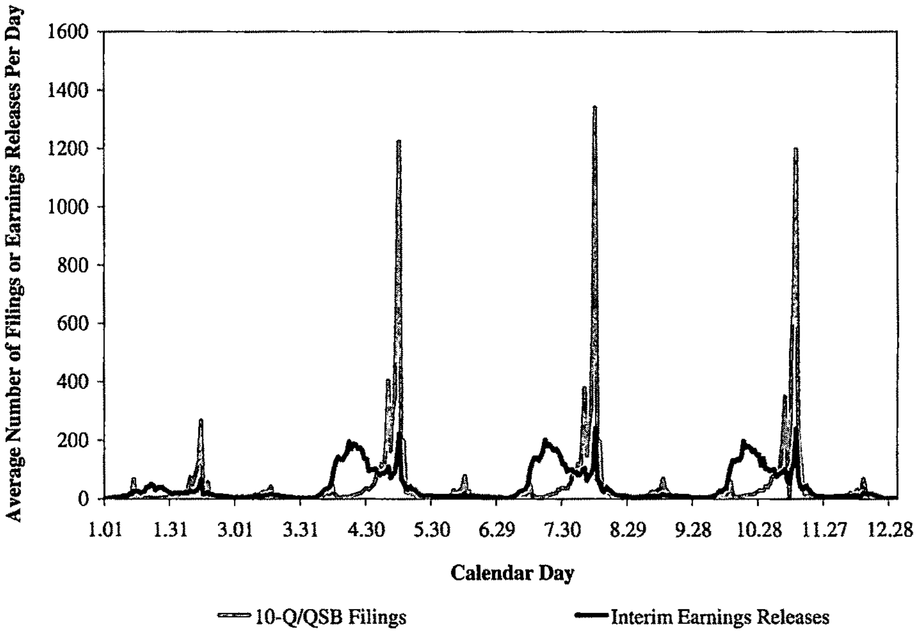
[6] We also exclude company names with "-pre FASB" in the Compustat database. After the promulgation of SFAS No. 94 *Consolidation of All Majority-Owned Subsidiaries*, Compustat created duplicate company records labeled "pre-FASB" based on voluntary company disclosures to provide unconsolidated data on a consistent historical basis.

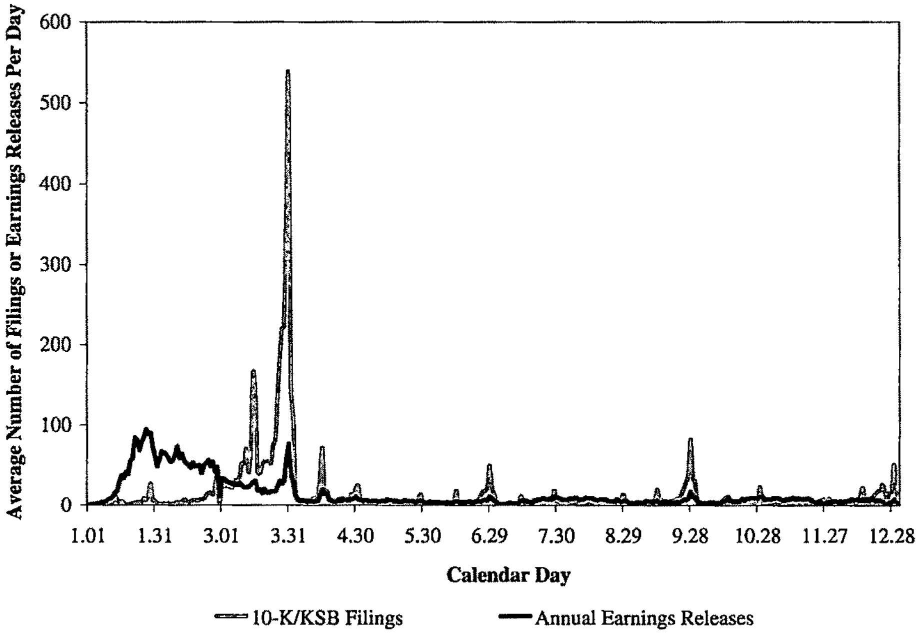
[7] Based on a manual verification of a random sample of fourth-quarter observations, Li and Ramesh (2008) find that the incidence of event-time clustering of earnings releases and periodic reports based on the Compustat database is overstated by 3.6 percentage points. However, this collection error does not misidentify periodic filings that follow an earnings announcement, which is the key subsample for this article.

FIGURE 1
Calendar Year Distribution of Periodic SEC Filings and Earnings Releases

Panel A: Distribution of 10-Q/QSB Filings and Interim Earnings Releases[a]



Panel B: Distribution of 10-K/KSB Filings and Annual Earnings Releases[b]



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[a] Frequencies represent times-series average of filings or interim earnings releases per day (based on the sample period 1996–2006).

[b] Frequencies represent times-series average of filings or annual earnings releases per day (based on the sample period 1996–2006).

In addition to the 10-Q/QSB filing dates, we also plot the interim quarter earnings release dates in Figure 1, Panel A (see also Cohen et al. 2007). Although the 10-Q/QSB filing dates have much larger spikes than the earnings release dates, both series show spikes approximately 45 calendar days after the end of the first three calendar-quarters. These spikes represent firms that bundle the first release of earnings information with the other information in periodic SEC reports.

Once again, consistent with Griffin (2003), we find that the 10-K/KSB filing dates have one large spike on March 31 (which is 90 days after the end of the fourth quarter of calendar-year firms), accounting for 10.5 percent of all 10-K/KSB filing dates in our sample. In addition, the four days ending on March 31 account for 24.9 percent of all 10-K/KSB filing dates in our sample (compared to 35.3 percent in Griffin [2003]). The calendar quarter-end clustering in annual filings has additional implications for market reaction tests, which we examine in Section IV. A smaller spike on March 15 is consistent with the accelerated filing requirements during the later part of our sample period. Similar to Figure 1, Panel A, we plot the fourth quarter earnings release dates in Figure 1, Panel B. Similar to the interim periods, while there are some concurrent earnings releases during the fourth quarter, the incidence is lower.[8] Untabulated analysis shows that 22.7 (16.4) percent of interim (fourth-quarter) earnings releases coincide with the SEC filing date during our sample period.

In most instances, investors first have access to an earnings press release announcing the preliminary earnings figures, followed by additional voluntary and mandated disclosures on the SEC filings date. This two-part information release provides a convenient vehicle for examining the *initial* market reaction to the earnings press release, as well as the investor *marginal* reaction to additional information provided in the periodic SEC reports. Our descriptive evidence suggests that concurrent earnings releases could potentially impact tests of market reaction to additional information provided in the periodic SEC reports.

III. PRIMARY EVIDENCE ON MARKET REACTION SURROUNDING PERIODIC SEC FILINGS

In this section, we first replicate the analysis in Griffin (2003) and show that univariate tests of market reaction around the filing of periodic SEC reports are affected by the phenomenon of concurrent earnings releases. Second, using a multivariate regression model, we control for concurrent earnings releases to provide evidence for the differential market reaction surrounding periodic filings.

Replication of Griffin (2003)

The stock return measure we examine is the standardized absolute excess returns (*SAER*) metric from Griffin (2003):

$$SAER_{it} = \frac{|er_{it}|}{\sigma(|er_{it}|)},$$

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[8] Some of the smaller sample studies have controlled for concurrent earnings releases when examining the information content of 10-K/10-Q filings. Although focused on the EDGAR phase-in period, Qi et al. (2000) eliminate observations in which the earnings announcements are within eight trading days of the 10-K filing date. Balsam et al. (2002) only consider 10-Q filings that are at least 12 trading days after the earnings announcement date, but do not find any reaction up to five days after the filing date. See also Foster and Vickrey (1978) and Foster et al. (1983).

where:

$|er_{it}|$ = absolute value of excess returns calculated as raw return of firm i for day t minus the value-weighted CRSP market return for day t; and

$\sigma(|er_{it}|)$ = standard deviation of the absolute value of excess returns of firm i over a ten-day estimation period adjoining the event period -5 to $+5$ (i.e., day -10 to day -6 and day $+6$ to day $+10$; Griffin 2003).

In Panel A of Table 1 we replicate Griffin (2003, Table 5, Panel C) using data from the period 1996–2006, which provide results of the relative market reactions around 10-Q filings. We focus on 10-Q filings in this section to illustrate the effect of concurrent earnings releases. Similar to Griffin (2003), the table reports results of paired t-tests comparing the *SAER* of a given event-day with several adjoining event-days. For instance, the number -0.012 at the top left side of the panel is the average of the *SAER* of day -3 minus that of day -4, which is statistically significant, indicating that the market reaction is lower on day -3 compared to day -4. Given that the two studies examine a similar calendar-time period, it is not surprising that our results are remarkably consistent with that of Griffin (2003). Specifically, the *SAER*s of days 0 and 1 are significantly higher than those for all of the other adjoining days reported in Panel A of Table 1.

To see the potential impact of concurrent earnings releases, we separately show the results for the 10-Q filings that follow a preliminary earnings announcement (Table 1, Panel B) and for the 10-Q filings made concurrently with the earnings release (Table 1, Panel C). These two categories comprise 81 percent and 19 percent of all 10-Q filings, respectively. A comparison of Panels A and B shows a dramatic difference in the market reaction around 10-Q filings. When we focus only on the 10-Q filings that follow preliminary earnings announcements, we find that the *SAER*s of days 0 and 1 are, in general, not significantly higher than those of the immediately preceding days. On the contrary, in six out of the eight comparisons, the day 0 and day 1 *SAER*s are significantly lower. In other words, after we exclude observations with concurrent earnings releases, we find that 10-Q filings elicit no incremental market reaction when compared to days preceding the event-days 0 and 1.[9]

Concurrent earnings releases appear to have a lesser impact when the *SAER*s of days 0 and 1 are compared to those of the days following them. While the *SAER*s of days 0 and 1 are significantly higher than those of the days following them, their economic significance is substantially lower. For these comparisons, the differences in the *SAER*s reported in Table 1, Panel B are, on average, more than 50 percent lower than those in Panel A.

In Table 1, Panel C we report the results for the 10-Q filings through which earnings information is released for the first time. In this case, we find large price reactions surrounding the filing dates (see also Easton and Zmijewski 1993). While these results are consistent with the prior research on the information content of earnings announcements, they also suggest the possibility that the market may react to information from periodic filings when the filings convey earnings information for the first time. Our analysis thus far suggests that even large-sample market reaction tests may be impacted by the systematic clustering of related events (see also Ecker et al. 2006). Using a multivariate regression model, we next control for concurrent earnings releases to provide evidence regarding the differential market reaction surrounding periodic filings.

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[9] Two observations are in order. First, untabulated analysis shows that the R^2 from a regression of *SAER*s reported in Griffin (2003, Table 5) and our own calculations in Panel A of Table 1 is 0.96. However, the R^2 drops to 0.31 when Griffin's *SAER*s are regressed on those reported in Panel B of Table 1. Second, our results on the lack of a statistically significant market reaction to 10-Q filings that do not include earnings information for the first time have been replicated in a recent working paper (Lu 2006).

TABLE 1
Differences in Standardized Absolute Excess Return Surrounding Form 10-Q Filing Dates[a]

Panel A: All 10-Q Filings

	From Day							
To Day	−4	−3	−2	−1	0	1	2	3
−3	−0.012*							
−2	−0.014*	−0.003						
−1	0.004	0.016**	0.018**					
0	0.087**	0.098**	0.101**	0.083**				
1	0.046**	0.058**	0.061**	0.042**	−0.040**			
2	−0.055**	−0.043**	−0.040**	−0.059**	−0.142**	−0.101**		
3	−0.083**	−0.071**	−0.068**	−0.086**	−0.169**	−0.128**	−0.028**	
4	−0.100**	−0.088**	−0.086**	−0.104**	−0.187**	−0.145**	−0.045**	−0.018**
No. of Obs.	166,997	167,001	167,001	167,005	167,012	166,984	167,016	167,034

Panel B: 10-Q Filings Preceded by a Preliminary Earnings Announcement

	From Day							
To Day	−4	−3	−2	−1	0	1	2	3
−3	−0.016**							
−2	−0.019**	−0.004						
−1	−0.007	0.009	0.012*					
0	−0.023**	−0.006	−0.003	−0.015*				
1	−0.067**	−0.050**	−0.047**	−0.059**	−0.044**			
2	−0.101**	−0.085**	−0.082**	−0.094**	−0.079**	−0.035**		
3	−0.114**	−0.097**	−0.094**	−0.106**	−0.091**	−0.047**	−0.012*	
4	−0.127**	−0.110**	−0.107**	−0.119**	−0.104**	−0.060**	−0.025**	−0.013**
No. of Obs.	135,439	135,437	135,437	135,438	135,438	135,428	135,439	135,441

(continued on next page)

TABLE 1 (continued)

Panel C: 10-Q Filings with a Concurrent Earnings Release

	From Day							
To Day	−4	−3	−2	−1	0	1	2	3
−3	0.008							
−2	0.008	0.000						
−1	0.051**	0.044**	0.043**					
0	0.555**	0.547**	0.547**	0.503**				
1	0.530**	0.523**	0.525**	0.479**	−0.024			
2	0.144**	0.137**	0.138**	0.093**	−0.411**	−0.386**		
3	0.051**	0.043**	0.044**	0.000	−0.504**	−0.478**	−0.094**	
4	0.014	0.006	0.008	−0.038**	−0.540**	−0.514**	−0.129**	−0.036**
No. of Obs.	31,558	31,564	31,564	31,567	31,574	31,556	31,577	31,593

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*, ** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

[a] This table is based on the sample period 1996–2006. Standardized absolute value of excess return (*SAER*) is calculated as $|AER|_{i,t}/\sigma|AER|_{i,t}$, where $|AER|_{i,t}$ is the absolute value of return $R_{i,t}$ in excess of the value-weighted CRSP market return $R_{m,t}$ for firm i on day t, and $\sigma|AER|_{i,t}$ is the standard deviation of $|AER|_{i,t}$ calculated over a ten-day estimation period (i.e., Day 10 to Day 6 and Day +6 to Day +10). The difference in standardized absolute value of excess return is calculated as the mean *SAER* of "To Day" (i.e., the row day) minus the mean *SAER* of "From Day" (i.e., the column day). Tests of the difference in standardized absolute value of excess return is based on a paired t-test. 10-Q Filings with a Concurrent Earnings Release are the 10-Q reports that are filed on the same day when earnings information is released to the market for the first time.

Multivariate Tests of Market Reaction Surrounding Periodic SEC Filings

We employ Easton and Zmijewski's (1993) rank test to examine the information content of periodic SEC reports (see also Carter and Soo 1999). Their purpose in using a ranking transformation is to obtain certain distributional properties appropriate for conventional statistical testing. In addition, Corrado (1989) and Campbell and Wasley (1996) suggest that the nonparametric rank tests are better specified and more powerful than the parametric t-tests for detecting abnormal daily returns and trading volume. While we report nonparametric test results, our inferences hold when our analyses are conducted using parametric statistics.

We develop a multiple regression model to jointly examine market reactions to periodic filings and earnings announcements:

$$DV_{it} = \alpha + \sum_{Event} \sum_{Period} \sum_{SB} \sum_{Day=-5}^{+5} \beta^{Event,Period,SB,Day} \cdot Dummy_{it}^{Event,Period,SB,Day} + \varepsilon_{it}, \quad (1)$$

where *DV* is a measure of daily stock price reaction (defined below). The independent variables are 264 indicator variables based on six types of information events as defined below (*Event*), two categories of reporting periods (interim versus fourth quarter), two levels of registrant type as in Griffin (2003) (SB versus non-SB), and 11 event-days (−5 to +5).

As in Easton and Zmijewski (1993) and Carter and Soo (1999), *DV* is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily absolute value of raw returns minus value-weighted CRSP market returns (Blom 1958). We assign each earnings announcement date and periodic filing date to one of the following six event categories to examine the effect of concurrent earnings releases (see also Easton and Zmijewski 1993):

1. Earnings announcement date when it precedes the filing of the periodic SEC report;
2. Periodic SEC report date when the earnings announcement date precedes the filing of the periodic SEC report;
3. Earnings announcement date when it is made after the filing of the periodic SEC report;
4. Periodic SEC report date when the earnings announcement is made after the filing of the periodic SEC report;
5. Earnings press release date when the press release is issued concurrently with the periodic SEC report; and
6. Periodic SEC report filing date when the SEC report is filed without an earnings press release for the fiscal quarter.[10]

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[10] Amir and Livnat (2005) make an important observation regarding the change in the definition of the earnings announcement date (RDQE) variable in the Compustat database. Prior to 1999, the Compustat RDQE variable was coded as missing when a company did not issue an earnings press release. Our discussions with Standard & Poor's indicate that, subsequent to 1999, Compustat's RDQE variable identifies the earliest date when earnings information becomes public knowledge. Therefore, subsequent to the calendar year 1999, Compustat would have included the SEC filing date as the "earnings announcement" date for firms that did not issue a press release. Consequently, the estimate of the incidence of concurrent earnings announcements is likely to be biased upward subsequent to 1999, i.e., firms in category 6 are likely to be included in category 5 beginning sometime in 1999. Given that the focus of our analysis is on the information content of category 2, any misclassification between categories 5 and 6 is not germane to our analysis. However, we re-calculated Table 2 data using only observations from the period for which we can cleanly distinguish between categories 5 and 6 (1996–1998). The tenor of our results is unaffected when the analysis is based on the smaller subsample.

We include all trading days during the period 1996–2006 corresponding to all firm-quarters with a periodic SEC filing or an earnings announcement date in the Compustat database. The intercept term captures the residual event category, and the incremental market reaction is measured using the slope estimates for the dummy variables that capture the marginal market reaction over and above the reference group.

The results of estimating Equation (1) are reported in Table 2. Note that the periodic filings have been separated into four subcategories (columns (1), (3), (5), and (6)). The results for annual earnings announcements and annual SEC reports are consistent with prior research (Easton and Zmijewski 1993). In general, both of these events generate price reactions regardless of whether the events coincide in calendar-time or whether one event precedes or follows the other.[11] In other words, concurrent earnings releases by themselves do not appear to impact inferences regarding the market reaction to 10-K filings. While the price reactions in the pre-event periods are consistently below the reference groups, those for day 0 through day 5 are significantly positive. The results for the 10-KSB filings are similar, although the statistical significance is much weaker.

Regardless of their timing, interim earnings releases elicit market reaction. With respect to 10-Q filings, we find consistently lower price reactions during the 11-day event period surrounding 10-Q filings made after earnings announcements, which account for roughly 80 percent of all 10-Q filings.[12] In terms of economic significance, unreported analysis shows that days -3 through $+5$ of 10-Q filings are among the least price-reactive days within the 265 days (264 event-days plus one residual category) examined in this study. When 10-QSB filings follow earnings announcements, all 11 event-days show "below-normal" price reactions, and four of them are significantly negative at conventional levels. Taken together, when annual (quarterly) SEC reports are preceded by earnings announcements, we find a significantly higher (lower) market reaction surrounding the filing of periodic reports of non-SB filers. While the results are similar, they are less pronounced for 10-KSBs. Untabulated analysis shows that if concurrent earnings announcements are not controlled for in Equation (1), we find statistically significant and larger price reactions surrounding the filing of all four types of periodic reports that we study.

Consistent with the extant literature we also examine the abnormal trading volume surrounding the filing of periodic reports to infer information content (e.g., Beaver 1968; Bamber 1986; Bamber 1987; Stice 1991; Cready and Mynatt 1991; Kim and Verrecchia 1991; Dontoh and Ronen 1993; Landsman and Maydew 2002; Cready and Hurtt 2002). We re-estimate Equation (1) by substituting a Blom transformation of Cready and Mynatt's (1991) volume-based metric. Inferences based on our volume-based results (untabulated) are virtually identical to our price-based inferences, except that there is no statistically significant volume reaction surrounding the filing of 10-KSB reports that are preceded by a preliminary earnings announcement. In summary, concurrent earnings releases have a

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[11] Consistent with Stice (1991), we find that earnings announcements that follow the corresponding periodic filing elicit statistically significant price reactions, which suggest that either firms are providing new disclosures in the earnings announcements, or that the market reacts more to a visible disclosure such as a press release.

[12] When 10-Q filings follow earnings announcements, Easton and Zmijewski (1993) find consistently below normal price reactions during days -2 to $+2$ for 10-Q filings of NYSE/AMEX firms, and for days 0 to $+2$ of NASDAQ firms. However, they find only day 0 price reaction of NYSE/AMEX firms to be significant at conventional levels.

TABLE 2
Regression Tests of Market Reaction Surrounding Periodic SEC Reports Based on Standardized Absolute Excess Return[a,b]

Panel A: 10-K

	EAD < FD		EAD > FD			
Day	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)	Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
Number of Events	46,998		1,030		4,448	1,597
−5	−0.018**	0.012*	0.034	−0.012	0.025	0.018
−4	−0.014**	0.011*	0.037	0.022	0.022	−0.029
−3	−0.021**	0.015**	0.007	0.065	0.012	−0.005
−2	−0.021**	0.028**	−0.051	0.067	0.023	0.008
−1	−0.010*	0.096**	−0.009	0.241**	0.054**	0.066**
0	0.041**	0.538**	0.011	0.200**	0.336**	0.065**
1	0.066**	0.645**	0.154**	0.116**	0.419**	0.094**
2	0.060**	0.149**	0.082*	0.140**	0.190**	0.046
3	0.037**	0.058**	0.003	0.138**	0.127**	0.044
4	0.031**	0.011*	−0.041	0.054	0.093**	0.032
5	0.012	−0.001	−0.012	0.060	0.045**	0.026

Panel B: 10-KSB

	EAD < FD		EAD > FD			
Day	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)	Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
Number of Events	2,767		510		1,343	781
−5	0.013	0.018	0.018	−0.073	0.008	0.021
−4	−0.004	0.006	0.042	−0.067	−0.040	0.004
−3	−0.021	0.001	0.037	0.026	−0.010	−0.017
−2	−0.017	0.008	0.019	−0.029	0.022	0.003

(continued on next page)

TABLE 2 (continued)

Day	EAD < FD		EAD > FD		Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)		
−1	0.002	0.095**	0.041	0.035	0.049	0.007
0	0.021	0.390**	0.031	0.217**	0.214**	0.073*
1	0.023	0.285**	0.174**	0.027	0.251**	0.081*
2	0.034	0.113**	0.168**	−0.003	0.136**	0.058
3	0.061**	0.054**	0.080	0.042	0.198**	0.005
4	0.027	0.067**	0.014	0.069	0.088**	0.005
5	0.036	0.022	0.009	0.054	0.122**	−0.019

Panel C: 10-Q

Day	EAD < FD		EAD > FD		Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)		
Number of Events	135,791		4,286		22,679	5,130
−5	−0.026**	0.037**	0.002	−0.003	0.003	0.016
−4	−0.036**	0.029**	−0.031	−0.011	−0.003	0.011
−3	−0.050**	0.040**	−0.002	0.005	0.004	−0.015
−2	−0.059**	0.053**	0.014	−0.036	0.000	−0.011
−1	−0.055**	0.127**	0.024	0.049*	0.048**	−0.010
0	−0.044**	0.572**	0.097**	0.129**	0.445**	0.008
1	−0.031**	0.662**	0.142**	0.067**	0.400**	0.043**
2	−0.048**	0.150**	0.039	0.051*	0.101**	0.013
3	−0.052**	0.074**	−0.018	0.033	0.043**	−0.008
4	−0.056**	0.044**	−0.008	0.003	0.019**	−0.020
5	−0.053**	0.028**	0.020	0.010	0.006	0.019

(continued on next page)

TABLE 2 (continued)

Panel D: 10-QSB

Day	EAD < FD		EAD > FD		Concurrent Earnings Announcement and Filing	Filings Without Earnings Announcement
	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)	(5)	(6)
Number of Events	6,568		1,895		5,255	2,483
−5	−0.011	0.023	0.002	−0.006	−0.026	0.005
−4	−0.010	0.026*	0.027	0.008	−0.030*	0.014
−3	−0.048**	0.054**	0.032	−0.018	0.000	0.008
−2	−0.035**	0.072**	−0.002	−0.004	0.011	0.023
−1	−0.050**	0.107**	0.010	0.121**	0.011	−0.014
0	−0.025*	0.374**	0.040	0.132**	0.207**	0.019
1	−0.015	0.276**	0.110**	0.112**	0.229**	0.044
2	−0.024	0.095**	0.017	0.087**	0.088**	0.010
3	−0.038**	0.087**	−0.054	0.060*	0.029*	0.052*
4	−0.013	0.035**	−0.047	0.064*	0.044**	0.029
5	−0.014	0.059**	−0.098**	0.032	0.027	0.009
Intercept				−0.019**		
Adj. R^2				0.0093		
Number of Obs.				16,428,983		

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*, ** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

[a] This table is based on the sample period 1996–2006. The independent variables are 264 dummy variables that are based on two levels of registrant type (SB versus non-SB), two categories of reporting periods (interim versus fourth quarter), six event categories (Columns (1) through (6)), and 11 event-days [−5 to +5] centered on the periodic filing date. EAD < (>) FD means earnings announcement date is before (after) the filing date. The numbers reported are regression slope estimates.

[b] The dependent variable is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily absolute value of raw returns minus value-weighted CRSP market return (Blom 1958).

substantial impact on the tests of market reactions surrounding the release of quarterly periodic reports, and to some extent, the 10-KSB reports.[13]

IV. CALENDAR-TIME CLUSTERING IN THE FILING OF 10-K REPORTS

The puzzling nature of our results thus far is that unlike for other periodic reports, the statistical significance of both the price and volume reactions surrounding the release of 10-K reports are robust to controls for concurrent earnings announcements. An interesting institutional phenomenon is that due to the mandatory filing deadlines during a substantial majority of our sample period, as well as firms' choice of fiscal year-ends (Smith and Pourciau 1988), 10-K filings cluster in calendar-time, with nearly 26 percent of all 10-K filings made within five trading days (-2, $+2$) surrounding calendar quarter-ends. We explore several reasons why market reaction surrounding 10-K reports may be due to filings that cluster around calendar quarter-ends.

Calendar Quarter Clustering in 10-K Reports and Market Reaction

First, we examine whether the market reaction surrounding the issue of 10-K reports corresponds to the finance literature regarding the behavior of asset prices and trading volume around the end of calendar quarters (e.g., Musto 1999; Carhart et al. 2002; Morey and O'Neal 2006). Given that portfolio holdings and returns are disclosed on a quarterly basis, mutual fund managers and other institutional investors have incentives to trade near quarter-end for various reasons (Meier and Schaumburg 2006).[14] Lakonishok et al. (1991) find that at quarter-ends, pension fund managers are more likely to sell underperforming stocks to get rid of "mistakes" from their portfolio disclosures ("window dressing"). Similarly, by trading just before a quarter-end, a fund manager can ensure that his/her holdings include some of the recently successful stocks, and exclude some stocks that had performed poorly to portray a certain level of portfolio risk exposure.

In addition to incentives for window dressing, a second stream of research finds that fund managers inflate quarter-end portfolio net asset values with last-minute purchases of stocks already held (Carhart et al. 2002). By temporarily pumping up the stock prices (or "marking the close"), the best-performing fund managers have incentives to show an even higher level of performance ("leaning for the tape") in order to benefit from the convex relationship between performance and cash inflows into mutual funds. Collectively, the incentives to window dress and "lean for the tape" are likely to increase trading volume and the magnitude of price reactions around calendar quarter-ends.

To provide evidence for the calendar-quarter effect, we first estimate the following regression model as in Carhart et al. (2002) using all observations during our sample period:

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[13] The tenor of our results in Table 2 is unaffected when we use standardized unexpected absolute returns as the dependent variable (Asthana and Balsam 2001) instead of the Z-transformation of nonparametric rank scores. The same is true of our unreported volume analysis as well. In addition, we also replicated the parametric returns analysis using market-model-adjusted abnormal returns (based on CRSP value-weighted return as a proxy for market return) with identical inferences.

[14] Mutual funds are required to disclose their holdings to shareholders at least semi-annually under the Investment Company Act of 1940 (Meier and Schaumburg 2006). To the extent mandatory disclosure impacts trading behavior, the regulation could create incentives for increased trading intensity at the end of semi-annual reporting periods (typically, June 30 and December 31). However, Ge and Zheng (2006) indicate that more than 50 percent of mutual funds voluntarily provide disclosures at quarterly intervals. In addition, extant literature suggests that all calendar quarter-ends create trading incentives due to "their prominence in the press (e.g., the *Wall Street Journal*'s quarterly pullout section), in shareholder reports (e.g., the quarterly mailings to pension-plan participants)" (Carhart et al. 2002).

$$DV_{it} = \alpha + \beta_1 YEND_{it} + \beta_2 YBEG_{it} + \beta_3 QEND_{it} + \beta_4 QBEG_{it} + \beta_5 MEND_{it} + \beta_6 MBEG_{it} + \varepsilon_{it}, \quad (2)$$

where DV_{it} is the Z-transformation of the percentile scores for absolute excess returns or trading volume, $YBEG_{it}$ ($YEND_{it}$) equals 1 when t is the first (last) trading day of a calendar year, $QBEG_{it}$ ($QEND_{it}$) equals 1 when t is the first (last) trading day of a calendar quarter that is not the first (last) quarter, and $MBEG_{it}$ ($MEND_{it}$) equals 1 when t is the first (last) trading day of a calendar month that is not the first (last) month of a calendar quarter. The regression results are provided in Table 3.

As in Carhart et al. (2002), we find significant price reactions at the end and the beginning of calendar quarters, although the results are less economically significant at month ends that are not calendar quarter-ends. We find similar results for trading volume, except that there is no consistent evidence of higher trading volume at the beginning of calendar quarters. Overall, our results illustrate the potential issues associated with measuring market reaction to events that are clustered around calendar quarter-ends.[15]

Of all the periodic SEC reports that are preceded by earnings announcements, we find a consistent market reaction surrounding only the 10-K reports. Consequently, we focus on

TABLE 3
Market Return and Trading Volume Behavior at the Turn of Year, Quarter, and Month

	Stock Return		Trading Volume	
	Coeff.	t-stat	Coeff.	t-stat
Intercept	−0.005	−20.71**	−0.007	−28.38**
YEND	0.054	13.65**	0.222	57.08**
YBEG	0.251	65.00**	0.040	10.46**
QEND	0.119	52.94**	0.276	123.29**
QBEG	0.108	47.63**	−0.001	−0.36
MEND	0.009	6.12**	0.030	21.59**
MBEG	0.036	25.48**	0.049	35.11**
Adj. R^2	0.001		0.001	
Number of observations	16,428,983		16,428,983	

*, ** $p < 0.05$ and $p < 0.01$, respectively, two-tailed tests.

This table is based on the sample period 1996–2006. The dependent variable for Stock Return test is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily absolute value of raw returns minus value-weighted CRSP market return (Blom 1958). The dependent variable for Trading Volume test is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily number of shares traded as a percentage of total number of shares outstanding (Blom 1958).

Variable Definitions:
YEND = 1 if last trading day of a calendar year, and 0 otherwise;
YBEG = 1 if first trading day of a calendar year, and 0 otherwise;
QEND = 1 if last trading day of a calendar quarter other than the fourth quarter, and 0 otherwise;
QBEG = 1 if first trading day of a calendar quarter other than the first quarter, and 0 otherwise;
MEND = 1 if last trading day of a month which is not the last of a quarter, and 0 otherwise; and
MBEG = 1 if first trading day of a month which is not the first of a quarter, and 0 otherwise.

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[15] We replicate model (2) for the sample period 1962–1985 examined in Easton and Zmijewski (1993) and find abnormally high price and volume reactions only around the calendar year-ends and not in the first three quarter-ends.

this group in our analysis of calendar-time clustering. To mitigate the effects of concurrent earnings releases, we assign all 10-K filings that are preceded by an earnings announcement to two groups: filings with an earnings announcement made less than six trading days before the filing date (EA_Near) and the others. We then divide the 10-K reports in the second group into two categories: late filers and on-time filers.[16] We create a separate category for late filers (Late Filers) given that they typically face significant economic events that lead to delayed filings (Alford et al. 1994). Finally, to isolate any calendar-quarter effects, the on-time filings are segregated into those made within seven trading days (−3, +3) surrounding calendar quarter-ends (Quarter-End) versus all others (Non-Quarter-End).

Univariate t-tests of the Z-transformed percentile scores for absolute excess returns and trading volume are reported in Panel A of Table 4 for each of the four subgroups. For each event-day within each subgroup, we also provide the proportions of observations with annual earnings announcements (*%EA_K*), with quarterly earnings announcements (*%EA_Q*) and that fall on calendar quarter-ends (*%Q_End*).

The results for the EA_Near subsample suggest that the significant price and volume reactions are associated with the immediately preceding annual earnings announcement and/or the clustering of filings at calendar quarter-ends. For late filers, we note that at least part of the price and volume reactions could be due to the disclosure of significant economic events surrounding delayed filings (Alford et al. 1994). In addition, calendar-time clustering appears to be another factor associated with the increased price and volume activities in the Late Filers subsample. Among on-time filers, both price and volume reactions are confined to the subsample in which the 10-K filings are made at calendar quarter-ends.

To triangulate our analysis, we next examine the calendar quarter-end returns of Non-Quarter-End filers. In this analysis, we examine the market reaction surrounding calendar quarter-ends of the last two groups in Table 4, Panel A, the results of which are reported in Panel B. Specifically, we examine abnormal returns and trading volume during the event window (−5, +5) surrounding the calendar quarter-end closest to the corresponding filing date, whereas the event windows are constructed around the filing dates in Panel A. To isolate the quarter-end effects, we exclude a small subset of firms that announced earnings during our event window. Given that the results for quarter-end filings are consistent with Panel A, we focus on the results for the Non-Quarter-End filers reported in the bottom section of Panel B. Although these firms did not file any periodic reports around calendar quarter-ends, their stocks experienced abnormal returns and trading volume around calendar quarter-ends.

In Table 4, Panel C we formally test the incremental effect of periodic filings after controlling for calendar-quarter effects. We construct a sample of event-days 0 and 1 ("filing day") of all 10-K filings made by firms issuing a preliminary earnings announcement. We augment the sample by including both data on all the calendar quarter-ends during the sample period not included above, as well as the day after the calendar quarter-end for the same set of firms. Every firm-trading-day included in this sample can be classified in to one of the following five categories:

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[16] For our sample period, Late Filers include 10-K filings with: (1) filing date ≥ 92 calendar days from the fiscal year-end if either the fiscal year ended before December 15, 2003, or the firm is not an Accelerated Filer; (2) filing date ≥ 77 calendar days from the fiscal year-end if the fiscal year of an Accelerated Filer ended between December 15, 2003 and December 15, 2006. See SEC Rules "Acceleration of Periodic Report Filing Dates and Disclosure Concerning Website Access to Reports" (September 5, 2002) and "Temporary Postponement of the Final Phase-In Period for Acceleration of Periodic Report Filing Dates" (November 17, 2004) for more details.

TABLE 4
Market Reaction Surrounding 10-K Filings and the Turn of Calendar Quarter Effect[a,b,c,d]

Panel A: Market Reaction around 10-K Filings

	Absolute Excess Returns		Trading Volume				
Day	**Rank Score**	**t-stat**	**Rank Score**	**t-stat**	*%EA_K*	*%EA_Q*	*%Q_End*
EA_Near Filings (Obs. = 2,820)							
−5	0.059	3.03**	0.081	4.20**	17.4%	0.0%	0.5%
−4	0.172	8.60**	0.213	10.93**	15.0%	0.0%	0.7%
−3	0.186	9.18**	0.272	13.90**	17.6%	0.0%	0.3%
−2	0.159	7.56**	0.361	18.78**	20.4%	0.0%	1.0%
−1	0.241	12.04**	0.448	23.40**	29.6%	0.1%	7.6%
0	0.207	10.22**	0.395	20.77**	0.0%	0.0%	14.4%
1	0.101	5.19**	0.287	15.55**	0.0%	0.0%	8.3%
2	0.086	4.65**	0.203	11.18**	0.0%	0.0%	4.3%
3	0.034	1.75	0.160	8.81**	0.0%	0.0%	2.3%
4	0.018	0.96	0.108	6.01**	0.0%	0.1%	1.7%
5	−0.007	−0.36	0.094	5.22**	0.0%	0.1%	2.2%
Late Filings (Obs. = 3,890)							
−5	0.010	0.65	0.009	1.18	0.0%	0.1%	0.9%
−4	0.050	3.21**	−0.018	−2.41*	0.0%	0.1%	1.1%
−3	0.056	3.57**	−0.028	−3.83**	0.0%	0.0%	1.1%
−2	0.016	0.96	−0.041	−5.49**	0.0%	0.1%	1.9%
−1	0.078	4.75**	−0.022	−2.97**	0.0%	0.1%	30.7%
0	0.143	8.86**	0.094	12.83**	0.0%	0.7%	10.8%
1	0.163	9.62**	0.092	12.44**	0.0%	0.3%	5.1%
2	0.048	2.93**	0.075	10.24**	0.0%	0.4%	3.1%
3	0.123	7.24**	0.074	10.06**	0.0%	0.4%	2.6%
4	0.051	3.24**	−0.004	−0.60	0.0%	0.4%	1.4%
5	0.009	0.55	−0.006	−0.87	0.0%	0.8%	1.2%

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TABLE 4 (continued)

Day	Absolute Excess Returns		Trading Volume		%EA_K	%EA_Q	%Q_End
	Rank Score	t-stat	Rank Score	t-stat			
Quarter End Filings (Obs. = 17,399)							
−5	−0.028	−3.78**	0.009	1.18	0.0%	0.0%	0.0%
−4	−0.025	−3.46**	−0.018	−2.41*	0.0%	0.0%	0.0%
−3	−0.067	−8.76**	−0.028	−3.83**	0.0%	0.0%	0.4%
−2	−0.050	−6.62**	−0.041	−5.49**	0.0%	0.0%	0.5%
−1	−0.035	−4.62**	−0.022	−2.97**	0.0%	0.0%	7.7%
0	0.052	6.75**	0.094	12.83**	0.0%	0.0%	34.5%
1	0.085	10.95**	0.092	12.44**	0.0%	0.0%	27.9%
2	0.119	15.56**	0.075	10.24**	0.0%	0.0%	18.2%
3	0.036	4.53**	0.074	10.06**	0.0%	0.0%	10.8%
4	0.048	6.24**	−0.004	−0.60	0.0%	0.0%	0.0%
5	0.011	1.40	−0.006	−0.87	0.0%	0.1%	0.0%
Non-Quarter End Filings (Obs. = 22,821)							
−5	−0.019	−2.95**	0.000	0.07	0.0%	0.0%	0.2%
−4	−0.024	−3.75**	−0.015	−2.40*	0.0%	0.0%	0.2%
−3	−0.025	−3.93**	−0.022	−3.42**	0.0%	0.0%	0.0%
−2	−0.029	−4.47**	−0.006	−0.91	0.0%	0.0%	0.0%
−1	−0.036	−5.52**	−0.019	−3.02**	0.0%	0.0%	0.0%
0	−0.012	−1.90	−0.021	−3.29**	0.0%	0.0%	0.0%
1	−0.008	−1.22	−0.004	−0.67	0.0%	0.0%	0.0%
2	−0.014	−2.11*	0.012	1.80	0.0%	0.0%	0.0%
3	−0.018	−2.80**	−0.022	−3.40**	0.0%	0.0%	0.0%
4	−0.007	−1.04	−0.007	−1.12	0.0%	0.0%	6.4%
5	−0.004	−0.61	0.007	1.04	0.0%	0.0%	6.4%

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TABLE 4 (continued)

Panel B: Market Reaction around Calendar Quarter End

	Absolute Excess Returns		Trading Volume			
Day	**Rank Score**	**t-stat**	**Rank Score**	**t-stat**	***%FILE_K***	***%FILE_Q***
Quarter End Filers (Obs.=17,299)						
−5	−0.022	−2.91**	−0.024	−3.30**	0.0%	0.0%
−4	−0.052	−7.04**	−0.064	−8.63**	0.0%	0.0%
−3	−0.094	−11.96**	−0.043	−5.69**	10.8%	0.0%
−2	−0.011	−1.48	−0.041	−5.52**	18.3%	0.0%
−1	−0.013	−1.69	−0.007	−1.00	27.9%	0.0%
0	0.143	17.94**	0.273	37.17**	34.6%	0.0%
1	0.093	12.00**	0.021	2.91**	7.6%	0.0%
2	0.151	20.22**	0.083	11.36**	0.5%	0.0%
3	−0.059	−7.49**	0.020	2.77**	0.4%	0.0%
4	0.027	3.52**	−0.044	−5.91**	0.0%	0.0%
5	0.022	2.95**	−0.028	−3.88**	0.0%	0.0%
Non-Quarter End Filers (Obs. = 21,483)						
−5	−0.016	−2.29*	0.002	0.28	6.7%	0.1%
−4	−0.075	−11.26**	−0.133	−19.12**	6.8%	0.1%
−3	−0.084	−12.38**	−0.092	−13.55**	0.0%	0.0%
−2	0.022	3.24**	0.006	0.95	0.0%	0.0%
−1	−0.020	−2.92**	−0.009	−1.29	0.0%	0.1%
0	0.070	10.03**	0.239	36.89**	0.0%	0.0%
1	0.126	18.33**	0.091	14.50**	0.0%	0.1%
2	0.127	18.56**	0.129	20.05**	0.0%	0.1%
3	−0.011	−1.62	0.056	8.60**	0.0%	0.2%
4	0.014	2.04*	−0.033	−4.97**	0.2%	0.2%
5	−0.015	−2.28*	−0.051	−7.81**	0.2%	0.3%

(continued on next page)

TABLE 4 (continued)

Panel C: Regression of Market Reaction around 10-K Filings and Calendar Quarter End

		Absolute Excess Returns			Trading Volume		
	n	**Coeff.**	**t-stat (clustered by firm)**	**t-stat (clustered by day)**	**Coeff.**	**t-stat (clustered by firm)**	**t-stat (clustered by day)**
Both Day 0 and Day 1 (Obs. = 376,323)							
QEND not on Filing Days	146,273	0.101	19.67**	3.02**	0.288	48.51**	5.41**
QEND on Filing Days	10,855	0.182	16.63**	2.81**	0.273	26.60**	3.97**
QBEG not on Filing Days	149,647	0.156	32.14**	4.68**	0.033	5.99**	0.69
QBEG on Filing Days	7,343	0.053	4.25**	0.83	0.022	1.80	0.55
Filing Day not on either *QEND* or *QBEG*	62,205	−0.006	−1.37	−0.42	−0.006	−1.26	−0.42
Comparing *QEND* on and not on Filing Day							
Control for clustering by firm			p-value = 0.000			p-value = 0.138	
Control for clustering by trading day			p-value = 0.171			p-value = 0.839	
Comparing *QBEG* on and not on Filing Day							
Control for clustering by firm			p-value = 0.000			p-value = 0.342	
Control for clustering by trading day			p-value = 0.086			p-value = 0.842	

(*continued on next page*)

TABLE 4 (continued)

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*, ** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

[a] This table is based on the sample period 1996–2006. We consider only 10-K filings that are preceded by earnings announcements. The metric for Absolute Excess Returns is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily absolute value of raw returns minus value-weighted CRSP market return (Blom 1958). The metric for Trading Volume is a Z-transformation of rank scores calculated for each calendar year and each firm based on the daily number of shares traded as a percentage of total number of shares outstanding (Blom 1958). Day 0 of a calendar quarter-end event refers to the last trading day of the calendar quarter.

[b] In Panel A, the event under examination is the 10-K filing. EA_Near Filings are the filings that are made within five trading days after the earnings announcements. Late Filings are those that are preceded by a preliminary earnings announcement by at least 6 trading days and satisfy either of the following two conditions: (1) filing date ≥ 92 calendar days from the fiscal year-end if either the fiscal year ended before December 15, 2003 or the firm is not an Accelerated Filer; (2) filing date ≥ 77 calendar days from the fiscal year-end if the fiscal year of an Accelerated Filer ended between December 15, 2003 and December 15, 2006. Quarter End filings are defined as filings that are neither EA_Near nor Late Filings and are made within a [−3, +3] trading-day window relative to the last trading day of any calendar quarter. Non-Quarter End filings are defined as filings that are neither EA_Near nor Late Filings and are not made within a [−3, +3] trading-day window relative to the last trading day of any calendar quarter. *%EA_K* for an event-day is the proportion of observations that have an annual earnings announcement on the event-day. *%EA_Q* for an event-day is the proportion of observations that have a quarterly earnings announcement on the event-day. *%Q_End* for an event-day is the proportion of observations on the last trading day of any calendar quarter-end.

[c] In Panel B, the event under examination is the calendar quarter-end. Quarter End filer group includes the single closest calendar quarter-end that corresponds to the 10-K filings in the Quarter End filings group defined in Panel A. Non-Quarter End filers group includes the single closest calendar quarter-end that corresponds to the 10-K filings in the Non-Quarter End filings group defined in Panel A. In addition, any calendar quarter-end that is within five trading days of any earnings announcement is dropped. *%FILE_K* for an event-day is the proportion of observations that have a 10-K filing on the event-day. *%FILE_Q* for an event-day is the proportion of observations that have a 10-Q filing on the event-day.

[d] In Panel C, the events under examination are 10-K filing date and calendar quarter-end. The 10-K filing date event includes Day 0 and Day 1 of all the 10-K filings in the Quarter End and Non-Quarter End filings groups defined in Panel A. The calendar quarter-end event includes Day 0 and Day 1 of all the four calendar quarter-ends that fall in the same calendar year as the corresponding 10-K filing date in the Quarter End and Non-Quarter End filings groups defined in Panel A. *QEND* (*QBEG*) refers to the Day 0 (1) of calendar quarter-end. *QEND* on (not on) Filing Day is a dummy variable that takes value 1 if the observation is on the Day 0 of any calendar quarter-end and (but not) on either Day 0 or Day 1 of any 10-K filing date. *QBEG* on (not on) Filing Day is a dummy variable that takes value 1 if the observation is on the Day 1 of any calendar quarter-end and (but not) on either Day 0 or Day 1 of any 10-K filing date. Filing Day not on either *QEND* or *QBEG* is a dummy variable that takes value 1 if the observation is on either Day 0 or Day 1 of 10-K filing date but not on Day 0 or Day 1 of any calendar quarter-end. t-statistics have been adjusted for heteroscedasticity and either firm-specific or trading-day-specific clustering in the panel data.

1. Calendar quarter-end without an SEC 10-K filing;
2. Calendar quarter-end with an SEC 10-K filing;
3. Calendar quarter beginning without an SEC 10-K filing;
4. Calendar quarter beginning with an SEC 10-K filing; and
5. SEC filing date not included in any of the above.

We then regress our measures of absolute excess returns and trading volume on indicator variables representing each of the five categories. Consistent with our univariate results, we find a significant calendar quarter-end effect ("*QEND* not on Filing Days") both in price and volume reactions. In addition, regardless of whether standard errors are adjusted for firm clustering or calendar-time clustering, we do not find a significantly higher trading volume on the calendar quarter-ends with a 10-K filing compared with those without. In contrast, when the standard errors are controlled for clustering of firms, the absolute excess stock returns around calendar quarter-ends with a 10-K filing are significantly higher than at other times. The difference is insignificant, however, when we adjust standard errors for clustering of trading days. In other words, while there is a calendar quarter-end effect in both price and volume reactions, there is some evidence of an incremental price reaction when a 10-K is filed on a calendar quarter-end.

In a subsequent subsection we consider the possibility of increased market attention during times in which the market is inundated with 10-K reports. However, consistent with our univariate results, we find no significant market reaction surrounding 10-K reports not filed on a calendar quarter-end or beginning, which account for more than 75 percent of the 10-K reports in our regression sample. The tenor of our results for 10-Q filings is unaffected when we control for any possible middle-of-the-month clustering effects, although there is no extant research on such effects (untabulated).

Although our study is motivated by EDGAR, our analysis covers a time frame roughly a decade after its introduction. During this period, various institutional developments have taken place that could have impacted the market's attention to periodic reports. For instance, periodic reports in the 2003–2006 period began including new information mandated by the Sarbanes-Oxley Act, including information relating to internal control weaknesses under Sections 302 and 404, which have drawn increased market attention (e.g., see Ashbaugh-Skaife et al. 2007; Gupta and Nayar 2007; Hammersley et al. 2008; Beneish et al. 2008.). Consistent with this possibility, we find no significant incremental price or volume reactions around calendar quarter-ends when our analysis is limited to the 1996–2002 period, regardless of how the standard errors are calculated. By extending the sample period much beyond the initial adoption of EDGAR, our results could be affected by any added scrutiny of periodic reports due to the Sarbanes-Oxley Act.[17]

Characteristics of Firms Filing at Calendar Quarter-Ends and Market Reaction

We conduct a second set of analyses to examine whether firms with certain economic characteristics are more likely to file their 10-Ks around calendar quarter-ends, and how this self-selection might impact our inferences. Given that we can observe the market reaction of all firms, whether they file their 10-K around calendar quarter-ends, we use the following switching regression model to examine the effects of firm self-selection:

Calendar quarter-end filing choice:

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[17] In addition to regulatory changes, increased use of electronic tagging and the desire for implementing instantaneous algorithmic trading strategies by certain sophisticated investors could also contribute to heightening the market's attention to periodic reports in recent times.

$$
\begin{aligned}
CQE_i^* &= \beta' Z_i + u_i;\\
CQE_i &= 1 \text{ if } CQE_i^* > 0;\\
CQE_i &= 0 \text{ if } CQE_i^* \leq 0.
\end{aligned}
$$

Market reaction model:

$$
\begin{aligned}
IR_{1i} &= \gamma_1' 1 + \varepsilon_{1i} \text{ if } CQE_i = 1;\\
IR_{0i} &= \gamma_0' 1 + \varepsilon_{0i} \text{ if } CQE_i = 0;
\end{aligned}
$$

where CQE^* is a latent variable representing the characteristics associated with firms filing a 10-K report around calendar quarter-ends ($CQE = 1$) and non-calendar quarter-ends ($CQE = 0$), IR_{1i} (IR_{0i}) is a measure of stock returns or trading volume around the 10-K filings made on calendar quarter-ends (non-calendar quarter-ends), Z_i is a vector of exogenous firm characteristics, and 1 is a vector of ones ("intercept"). The sample is constructed from event-days 0 and 1 of Quarter-End Filings and Non-Quarter-End Filings in Panel A of Table 4 with loss of observations due to missing data on firm characteristics. We consider firm characteristics that are typically discussed in the disclosure literature (e.g., Sengupta 2004). In addition, we also include a dummy variable representing firms with calendar year-ends, as they are more likely to file their 10-Ks around calendar quarter-ends during most of our sample period, although our inferences are unaffected by its exclusion.

The switching regression results for stock returns and trading volume are presented in Panel A of Table 5.[18] The selection model suggests that calendar year-end firms, loss firms, and firms with high trading volume or stock return volatility, more often file around calendar quarter-ends. Large firms, firms with high institutional ownership, and firms with high book-to-market are less likely to file their 10-Ks on calendar quarter-ends. The second-stage regression results, which control for endogeneity, show that the market reaction on calendar (non-calendar) quarter-end filings is significantly positive (negative). Overall, our inferences regarding a significant calendar quarter-end effect remain valid after controlling for possible self-selection in the choice of filing date.

To provide additional evidence with regard to self-selection, we perform a robustness check to isolate the market reaction relating to firm effects versus calendar-time effects, following a reviewer's suggestion. Based on the first-stage regression model, we calculate the prediction error (actual outcome minus predicted probability) and assign one-third of firm-quarters with the most negative (positive) errors to the *LOW* (*HIGH*) group. Firm-quarters in the *LOW* group represent non-calendar quarter-end filings ($CQE = 0$) of firms whose characteristics indicate that they are more likely to file at calendar quarter-ends, while those in the *HIGH* group represent the opposite. The univariate price and volume analysis in Panel B of Table 5 shows an insignificant market reaction for *LOW* and a significantly positive market reaction for *HIGH*. This suggests that higher market reaction is strongly associated with the *actual* calendar timing of 10-K filing, rather than firms' characteristic predisposition.

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[18] We use a full information maximum likelihood (FIML) method (Lokshin and Sajaia 2004) to estimate the endogenous switching regressions. An alternative approach would be to use two separate second-stage regressions with correction for endogeneity through the inclusion of the Inverse Mills ratio. However, given that the Inverse Mills ratios are estimated, the residuals from the second-stage regressions cannot be used to estimate standard errors for the two-stage estimates. While the correct variance–covariance matrix can be estimated using Maddala's (1983, 223–228) procedure, the FIML method is more efficient and simultaneously estimates the first- and second-stage equations to yield consistent standard errors (Alene and Manyong 2007).

TABLE 5
Self-Selection and Turn-of-Calendar-Quarter Effect[a]

(n = 69,452)

Panel A: Two-Stage Switching Regressions

	Pred. Sign	Coeff.	z-stat
First Stage: Selection Model (Dependent Variable: *CQE*)[b]			
DEC_YEAR_END	+	0.761	27.10**
SPE	+	−0.094	−5.22**
EID	+	−0.117	−4.72**
LOSS	+	0.081	3.88**
HIGH_TECH	+	−0.064	−2.23*
VOL	+	−0.155	−6.08**
RET_VOLATILITY	+	9.478	17.17**
LN_MV	−	−0.099	−11.62**
INST	−	−0.555	−13.08**
ANA_FOLLOW	−	0.025	0.90
BM	?	−0.080	−3.78**
BIG8	−	0.158	3.56**
Intercept		−0.289	−4.30**
Pseudo R^2		0.1270	

	Price Reaction		Volume Reaction	
	Coeff.	z-stat	Coeff.	z-stat
Second Stage: Market Reactions Models[c]				
Calendar Quarter End Filers	0.092	5.69**	0.056	3.10**
Non-Calendar Quarter End Filers	−0.060	−5.19**	−0.086	−6.18**
Wald test of independent equations $\chi2(1)$	23.33 (p-value = 0.000)		36.87 (p-value = 0.000)	
Wald test of equality of coefficients $\chi2(1)$	59.07 (p-value = 0.000)		37.85 (p-value = 0.000)	

Panel B: Three Groups Analysis Based on First-Stage Prediction Error[d]

	Price Reaction		Volume Reaction	
	Rank Score	t-stat	Rank Score	t-stat
LOW	0.003	0.48	0.010	1.41
MEDIUM	−0.008	−1.13	−0.020	−2.61**
HIGH	0.069	9.80**	0.092	11.98**

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*, ** $p < 0.05$ and $p < 0.01$, respectively, two-tailed tests.

z-statistics have been adjusted for heteroscedasticity and firm-specific clustering in the panel data.

[a] This table presents the results from a switching regression model to examine the effect of self-selection on the market reaction to periodic SEC filing around calendar quarter-end. The switching regression model is estimated jointly using a maximum likelihood method. The sample includes the day 0 and day 1 of all the 10-K filings from 1996 to 2006 that are preceded by the corresponding earnings announcement by at least six trading days and are made within (1) 91 calendar days after the fiscal year-end if either the fiscal year ended before December 15, 2003 or the firm is not an Accelerated Filer; or (2) 76 calendar days after the fiscal year-end if the fiscal year of an Accelerated Filer ended between December 15, 2003 and December 15, 2006.

[b] In the First Stage Selection Model, the dependent variable, *CQE*, takes value of 1 if the 10-K filing is within a [−3, +3] trading-day window relative to the last trading day of any calendar quarter-end, and 0 otherwise.

(*continued on next page*)

TABLE 5 (continued)

DEC_YEAR_END is a dummy variable that takes the value of 1 if the fiscal year of the firm ends in December, and 0 otherwise. *SPE* is a dummy variable that takes the value of 1 if the firm reports any special item in the fourth quarter, and 0 otherwise. *EID* is a dummy variable that takes the value of 1 if the firm reports any extraordinary item or discontinued operation in the fourth quarter, and 0 otherwise. *LOSS* takes the value of 1 if the firm reports a net loss in the fourth quarter, and 0 otherwise. *HIGH_TECH* is a dummy variable that takes the value of 1 if the firm's quarter-end SICCD code from CRSP is 2833–2836 (Drugs), 8731–8734 (R&D Services), 7371–7379 (Programming), 3570–3577 (Computers), 3600–3674 (Electronics), or 3810–3845 (Precise Measurement Instruments), and 0 otherwise. *VOL* is the total shares traded in the fourth quarter scaled by total shares outstanding. *RET_VOLATILITY* is the standard deviation of daily stock returns over the fourth quarter (at least 20 days are required). *LN_MV* is the logarithm of market value at the fiscal year-end. *INST* is the percent of shares held by institutions as reported on Spectrum for fiscal year-end (if a firm is not covered by Spectrum, it is assumed to be 0). *ANA_FOLLOW* is a dummy variable that takes the value of 1 if at least one analyst is covering the firm as reported on I/B/E/S database, and 0 otherwise. *BM* is the book-to-market ration on the fiscal year-end. *BIG8* is a dummy variable that takes the value of 1 if the firm is audited by a Big 8 accounting firm, and 0 otherwise. *VOL*, *RET_VOLATILITY*, *LN_MV*, *INST*, and *BM* have been winsorized at top and bottom 0.5 percent.

[c] In the Second Stage Market Reaction Models, the dependent variable for the Price Reaction is a normalized rank score for Absolute Excess Returns calculated by first ranking daily Absolute Excess Returns over each calendar year for each firm and then converting the rank to a standard normal Z-statistic based on Blom (1958). Absolute Excess Return is the absolute value of return in excess of the value-weighted CRSP market return. Dependent variable for the Volume Reaction is a normalized rank score for Trading Volume calculated by first ranking daily Trading Volume over each calendar year for each firm and then converting the rank to a standard normal Z-statistic based on Blom (1958). Trading Volume is the number of shares traded divided by total number of shares outstanding. Apart from correction for endogeneity, the only independent variable in the Second Stage Market Reaction Models is the Intercept.

[d] Based on the first-stage regression model, we calculate the prediction error as actual outcome minus predicted probability and assign firm quarters into terciles with the *LOW* group consisting of the most negative prediction errors (i.e., non-calendar quarter-end filings ($CQE = 0$) predicted as calendar quarter-end filings), and *vice versa* for the *HIGH* group.

Market Reaction at Calendar Quarter-Ends and Information Transfer from 10-K Reports

Finally, we explore an alternative explanation for the market reaction that could arise due to information transfer from 10-K filings when they are clustered in calendar-time. In other words, during calendar quarter-ends, investors not only trade in the stock of the firms that filed the periodic reports (i.e., reaction to the company's own news), but also trade in the stocks of other firms facing similar economic circumstances (e.g., industry membership) as a reflection of any information transfer from the filings.

To test this possibility, we focus on the end of March, due to the greatest concentration of 10-K reports that occur at this calendar quarter-end. To test for information transfer, we regress the abnormal price and volume measures on an indicator variable that takes a value of 1 for observations from industry-years (based on four-digit SIC for each calendar year) with no annual reports filed three trading days surrounding the end of March (*Noinfo*), and a continuous variable representing the percentage of firms in the industry-year filing annual reports around the end of March (*%Mar*). We present results for multiple event windows around the end of March for which our return and volume metrics are re-calculated over the relevant event period.

The regression results of our information transfer analysis are presented in Table 6. Although the evidence is less consistent for volume regressions, the results for the *%Mar* variable suggest an intra-industry information transfer from announcing firms to non-announcing ones as a function of the intensity of information released during the end of

TABLE 6
Regression Analysis on Market Reaction Surrounding the End of March and Information Transfer from 10-K Reports[a,b]

(Observations = 32,948)

		Cumulative Absolute Excess Returns		Cumulative Trading Volume	
Day(s)		**Coeff.**	**t-stat**	**Coeff.**	**t-stat**
0	*Noinfo*	−0.024	−1.82	−0.027	−2.24*
	%Mar	0.154	4.66**	0.188	6.04**
	Intercept	0.048	3.38**	0.185	13.72**
0~1	*Noinfo*	−0.012	−0.96	−0.019	−1.66
	%Mar	0.113	3.52**	0.011	0.36
	Intercept	0.088	6.44**	0.185	14.38**
0~2	*Noinfo*	−0.031	−2.55*	−0.023	−1.96
	%Mar	0.134	4.38**	0.012	0.41
	Intercept	0.129	9.74**	0.190	14.87**
0~3	*Noinfo*	−0.009	−0.75	−0.013	−1.18
	%Mar	0.109	3.54**	0.056	1.91
	Intercept	0.089	6.72**	0.146	11.53**
0~4	*Noinfo*	−0.018	−1.49	−0.012	−1.00
	%Mar	0.180	5.99**	0.083	2.83
	Intercept	0.059	4.52**	0.097	7.58**

*,** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

t-statistics have been adjusted for heteroscedasticity and firm-specific clustering in the panel data.

[a] This table is based on the sample period 1996–2006. The event under examination is the end of March. The metrics for Cumulative Absolute Excess Returns and Cumulative Trading Volume are calculated according to the number of cumulative days. For example, for Days 0~4, a typical 252 trading days from each firm-calendar year are broken into 50 five-day intervals. The metric for Cumulative Absolute Excess Returns is a Z-transformation of rank scores calculated for each calendar year and each firm based on the five-day cumulative absolute value of daily excess return based on value-weighted CRSP market return (Blom 1958). The metric for Trading Volume is a Z-transformation of rank scores calculated for each calendar year and each firm based on the five-day cumulative shares turnover (Blom 1958). Day 0 refers to the last trading day of March.

[b] *%Mar* is calculated each year as the percentage of firms from each industry (four-digit SIC code) that filed 10-K reports around the end of March (i.e., within a [−3, +3] trading-day window relative to the last trading day of March). The calculation is based on the full population of the Merged Compustat/CRSP database. *Noinfo* takes the value of 1 if the firm belongs to an industry in which no firm filed its annual report around March end, and 0 otherwise. Only Non-March End 10-K filers are included in the regressions. Non-March End 10-K filers are defined as firms with 10-K filings that are preceded by a preliminary earnings announcement for at least six trading days and are not made within a [−3, +3] trading-day window relative to the last trading day of March.

March.[19] However, after controlling for information transfer, we still find a calendar quarter-end effect, as indicated by the positive and statistically significant intercept term in all event windows for both price and volume regressions. There is some evidence that the calendar quarter-end effect is less pronounced in firms from industries with no information release during the end of March. Overall, our evidence suggests that the increased market attention as well as the existence of intra-industry information transfer occurring when 10-Ks are bunched together at least partially explains the calendar quarter-end effect.[20]

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[19] We also consider the possibility (thanks to a reviewer) of a stronger information-transfer effect in the banking industry in which many firms face very similar economic circumstances and usually have calendar year-ends. Banks (per Fama-French's 48 industry classification) represent 10 percent of firm-quarters in the Table 6 analysis. We re-estimate the Table 6 regressions with a bank dummy, as well as interactions of the bank dummy with *Noinfo* and *%Mar*. Regression results indicate a larger and generally significant information transfer coefficient for banks (*bank* × *%Mar*) in the volume regressions, but the evidence of incremental information transfer is weaker at best in the price regressions.

V. ANALYST FORECAST REVISIONS SURROUNDING THE FILING OF PERIODIC REPORTS

In this section we examine the reaction of an important financial intermediary in the capital market, namely equity analysts, to the release of earnings information and the filing of periodic reports. If equity analyst behavior is similar to the overall market reaction, then we would expect a strong (no or limited) association between analyst forecast revisions and earnings announcements (the filing of periodic SEC reports).

For each firm we calculate the number of fiscal-year earnings forecast revisions on each trading day. We estimate model (1) using standardized daily count of analyst forecast revisions as the dependent variable.[21] The results of model (1) for the 10-K/Q filings using the intensity of analyst forecast revisions as the dependent variable are listed in Table 7. We find no evidence of a significant increase in analyst forecasting activity around 10-Q or 10-K filings. Thus, at least one important set of capital market participants does not respond contemporaneously in a pervasive fashion to periodic SEC filings that follow earnings announcements.

An important difference in the analyst reaction tests reported in Table 7, versus the market reaction tests reported in Table 2, pertains to the statistical significance of 10-K filings that are preceded by earnings announcements. While there is evidence of a significant market reaction (returns or volume) during the 10-K event window, the equity analysts do not react, i.e., the observed calendar-time clustering in market reaction does not appear to be present in the analyst community. We confirm this conjecture by replicating Panel A of Table 4 (unreported), finding no material difference in the level of analyst forecast revisions around 10-K reports filed near calendar quarter-ends versus others. In other words, we may rule out the possibility of a pervasive information-transfer effect in stock returns or trading volume due to analyst forecast revisions clustered in calendar-time.

VI. THE IMPLICATIONS OF DISCLOSURE ENDOGENEITY FOR FUTURE RESEARCH

Our study provides an example in which disclosure endogeneity affects large-sample tests of market or investor reaction surrounding specific corporate disclosures. While we focus on the periodic SEC reports and earnings announcements, other types of concurrent disclosures do in fact occur in capital markets (Ecker et al. 2006; Lansford 2006). From the disclosure theory standpoint, examining the economic incentives for simultaneous release of multiple information items by the same economic agent or different market participants could further enhance our understanding of the corporate disclosure landscape. The

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[20] In contrast, Hirshleifer et al. (2008) find that investors react less to earnings announcements when they face a large number of competing announcements on that day, and in addition, they find a stronger post-earnings announcement drift for these announcements. See also You and Zhang (2008).

[21] Given significant cross-sectional variation in analyst followings, we standardize the daily count by subtracting the calendar-year mean revisions, divided by the corresponding calendar-year standard deviation. The tenor of our results is unaffected when we use the nonparametric rank score as the dependent variable.

TABLE 7
Regression Tests of Analyst Reaction Surrounding Periodic SEC Reports Based on Standardized Number of Analyst Forecast Revision[a,b]

Panel A: 10-K

	EAD < FD		EAD > FD			
Day	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)	Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
−5	−0.018**	−0.061**	−0.051	−0.087	−0.026	−0.005
−4	−0.016**	−0.063**	0.032	−0.034	−0.041	0.011
−3	−0.015**	−0.061**	0.037	−0.002	−0.043	0.042
−2	−0.021**	−0.057**	−0.035	−0.121	−0.035	0.051
−1	−0.034**	−0.030**	0.114	−0.035	−0.016	0.063
0	−0.032**	0.283**	0.115	0.045	0.162**	0.014
1	−0.004	2.234**	0.213**	0.227**	0.977**	0.003
2	0.006	1.050**	0.103	0.105	0.440**	0.039
3	−0.003	0.412**	0.040	0.115	0.237**	−0.006
4	−0.001	0.208**	−0.009	−0.002	0.196**	0.052
5	0.002	0.122**	0.052	0.027	0.105**	0.040

Panel B: 10-Q

	EAD < FD		EAD > FD			
Day	Filing (1)	Earnings Announcement (2)	Filing (3)	Earnings Announcement (4)	Concurrent Earnings Announcement and Filing (5)	Filings Without Earnings Announcement (6)
−5	0.009**	−0.025**	0.070**	−0.030	−0.008	0.113**
−4	0.013**	−0.039**	0.089**	−0.034	−0.016	0.152**
−3	−0.005	−0.046**	0.021	0.007	0.000	0.128**
−2	−0.008**	−0.040**	0.057	−0.015	0.008	0.112**
−1	−0.015**	−0.009**	0.059	0.006	0.059**	0.124**
0	−0.027**	0.465**	0.152**	0.272**	0.478**	0.118**

(*continued on next page*)

TABLE 7 (continued)

	EAD < FD		EAD > FD			
Day	**Filing (1)**	**Earnings Announcement (2)**	**Filing (3)**	**Earnings Announcement (4)**	**Concurrent Earnings Announcement and Filing (5)**	**Filings Without Earnings Announcement (6)**
1	−0.006*	2.700**	0.406**	0.281**	1.718**	0.138**
2	−0.005	1.331**	0.417**	0.248**	0.870**	0.150**
3	0.003	0.524**	0.118**	0.132**	0.436**	0.061**
4	−0.007*	0.260**	0.049	0.174**	0.237**	0.048*
5	−0.012**	0.138**	0.009	0.127**	0.161**	0.044
Intercept			−0.069**			
Adj. R^2			0.1157			
Number of Obs.			11,171,931			

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*,** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

[a] This table is based on the sample period 1996–2006. Only firms covered by I/B/E/S are included in the sample. The independent variables are 264 dummy variables that are based on two levels of registrant type (SB versus non-SB), two categories of reporting periods (interim versus fourth quarter), six event categories (Columns (1) through (6)), and 11 event-days [−5 to +5] centered on the periodic filing date. EAD < (>) FD means earnings announcement date is before (after) the filing date. The results for 10-KSB and 10-QSB are suppressed. The numbers reported are regression slope estimates.

[b] The dependent variable is the standardized number of analyst forecast revision (SNAFR) based on the daily number of analyst forecast revision (NAFR) (only fiscal year one earnings forecast revisions (FPI = 1) are counted) reported by the I/B/E/S detail database. SNAFR is calculated as [NAFR − mean(NAFR)]/std(NAFR) for each calendar year and each firm.

implication for empirical work is that future research using market reaction tests could consider ways to systematically control for disclosure confluence (see also Ecker et al. [2006] for an early attempt.).

In particular, future research could develop and test theories regarding when investors are focused on periodic reports, and what disclosure channels draw the investors' attention to the filings. One possibility would be to explore the managerial choice of disclosure channels, and the market reaction as a function of the saliency of such disclosure channels (Bamber and Cheon 1998; Stice 1991). For instance, a comparison of columns (5) and (6) of Table 2 suggests that the market appears to react less to 10-Q reports that include earnings information for the first time if they are not accompanied by an earnings press release. One possibility is that when investors see a salient earnings press release, they may immediately focus on the additional information in the corresponding periodic filings.

While future research could explore disclosure saliency more thoroughly, we provide some preliminary analysis to highlight the importance of controlling for disclosure endogeneity, as well as the need to consider market microstructure level data in such tests. We discuss below two scenarios: (1) when the nature of the information in the periodic filings is likely to call for the use of a more salient complementary disclosure channel; and (2) when the market is likely to be focused on the periodic report due to the greater economic uncertainty faced by the company.

Regarding the first scenario, extant research finds that at the time of periodic filings, if firms downwardly revise the earnings number originally reported in the preliminary earnings announcement, then stock prices decline commensurably (Hollie et al. 2005, 2006). One possible explanation of this finding is that companies that downwardly revise their earnings figures face increased disclosure risk and, therefore, are more likely to provide a corrective disclosure using a channel that is as *salient* as the one used for the original disclosure (i.e., earnings press release).

To examine this possibility, we extensively review publicly available information for a sample of 151 firm-quarters in the period 1996–2003 with the largest downward earnings revisions identified from comparing earnings numbers from Compustat's Preliminary and As-First-Reported databases (similar to Hollie et al. 2005).[22] We eliminate 62 observations that did not represent real downward revisions for various Compustat data coding and data collection issues. Of the remaining 89 cases representing true downward revisions, firms used a preemptive press release in 50 instances to warn investors about the impending downward revision prior to the periodic report filing date. Additionally, in 36 cases, firms concurrently provided a press release to highlight the downward revision in the periodic report, leaving only three cases with no indication of a concurrent or preemptive salient disclosure. Further analysis indicates that price reaction surrounding the filing of the periodic report is limited to the 36 observations that included a concurrent press release (untabulated). While our preliminary analysis suggests that the choice of disclosure channel may be associated with the extent of market reaction, future research could potentially use market microstructure level data to isolate the channel that draws investor attention to the information in periodic reports.

In the second scenario, when companies face greater economic uncertainty, they are likely to be subject to greater market scrutiny, and consequently, provide disclosures to mitigate the effects of information risk. We suggest that studies examining market reactions to periodic reports may benefit from identifying and controlling for such endogenous disclosures when testing for market reactions to periodic reports or other mandated disclosures. We illustrate the effects of disclosure endogeneity using delayed periodic filings, when companies are expected to experience significant economic events (Alford et al. 1994). To examine the market reaction to delayed periodic reports, we use Callen et al.'s (2006) variance decomposition approach. Our market reaction analysis thus far focuses on aggregate stock price reactions surrounding the filing of periodic reports. The variance decomposition approach (Vuolteenaho 2002), however, can be used to isolate the extent to which excess stock returns around SEC periodic report dates generally are associated with changes in "cash-flow" expectations (the numerator effect) decomposed into cash flows and accruals versus changes in expected returns (the denominator effect).

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[22] Details of this untabulated analysis are available from the authors. To be consistent with Hollie et al. (2005), we limit our sample to the 1996–2003 period. The sample that we analyze is roughly one-third of the population of downward revisions identified based on the methodology in Hollie et al. (2005).

In Table 8, we report regression results in which dependent variables are the variance contributions of expected return news, cash flow news, and accrual news for event and non-event windows surrounding the filing of periodic reports.[23] Consistent with Callen et al. (2006), we use $(-1, +1)$ as the event window and consider $(-4, -2)$ and $(+6, +8)$ as alternative non-event windows. Our regression model includes indicator variables for the event window (*EVENT*), delayed filings (*OVERDUE*), delayed filings with a Form NT 10-K/Q submission (*NT*), and all of the relevant interaction terms among the dummy variables.[24] The main effect, *EVENT*, and the two interactions involving *EVENT* should capture the incremental effects during the event window.

Table 8 indicates that while there is evidence that the market appears to reassess discount rates at the time of 10-K filing (see slope of *EVENT* in the *VARNRF* model), the cash flow and accrual components do not exhibit a significant shift during the event period. The statistical significance of the intercepts in the earnings and accruals news models is consistent with the evidence on post-earnings-announcement drift and other earnings-based anomalies. The slope estimate for *OVERDUE* indicates a statistically and economically significant market reaction to cash flows and accruals information when firms tend to delay their filings. However, we find no consistent evidence of an incremental reaction during the event window, as indicated by the lack of significance of the variables that are interacted with *EVENT*.

One approach to jointly control for disclosure endogeneity and to parse out the effects of different "contemporaneous" disclosures is to rely on market microstructure level data. The use of microstructure-level data also offers opportunities to examine the increasing role of information intermediaries in mitigating the effects of information overload by sifting relevant information from corporate filings to different market participants. Future research could also study how the technological improvements unleashed by the Internet, EDGAR, and possibly XBRL in the future, impact the capital market information architecture and, more so, the speed of dissemination and assimilation of corporate disclosures.

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[23] We follow the sample construction procedure and empirical methodology described in Callen et al. (2006) for our sample period 1996–2006. Unreported results indicate that our descriptive evidence is very similar to that reported in Callen et al. (2006, Table 1). Also, similar to their work, we focus only on the variance components in our regressions.

[24] We acknowledge valuable input from a referee on the model specification. Given that *NT* is a subset of *OVERDUE*, we do not interact the two. The tenor of our results for expected return news is not affected when we separately analyze quarter-end and non-quarter-end filings, but the cash flow and accruals news components are not significant in the quarter-end filings.

TABLE 8
Variance Decomposition around Periodic SEC Filings (69,006 Firm-Quarters)[a,b]

Regression Results for Filing Date Cash Flow-Accrual Model Variance Decomposition

	VARNRF	*VARNCFF*	*VARNACF*
Event Days [−1, +1] versus Pre-Event Days [−4, −2]			
EVENT	0.000001**	0.000000	0.000001
OVERDUE	0.000001**	0.003433**	0.003246**
*OVERDUE * NT*	−0.000001*	0.002586	0.005646*
*EVENT * OVERDUE*	0.000000	0.000013*	−0.000013
*EVENT * OVERDUE * NT*	0.000001*	−0.000045*	0.000054
Intercept	0.000003**	0.012990**	0.011349**
Event Days [−1, +1] versus Post-Event Days [+6, +8]			
EVENT	0.000000*	0.000000	−0.000002
OVERDUE	0.000001**	0.003436**	0.003239**
*OVERDUE * NT*	−0.000001	0.002603	0.005657*
*EVENT * OVERDUE*	0.000000	0.000011	−0.000006
*EVENT * OVERDUE * NT*	0.000001*	−0.000061*	0.000043
Intercept	0.000004**	0.012989**	0.011352**

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*,** Significant at 5 percent and 1 percent level, respectively, two-tailed test.

All test statistics in regressions have been adjusted for heteroscedasticity and firm-specific clustering in the panel data.

[a] We follow the sample selection procedure in Callen et al. (2006) for our sample period. For a firm-quarter to be included in the sample, it must satisfy the following: (1) it is on the Preliminary and AFR Compustat quarterly databases between the first calendar quarter of 1996 and the fourth calendar quarter of 2006; (2) it has a fiscal quarter-end market value (DATA14 * CSHOQ) in excess of $1 million; (3) it has non-missing CUSIP and earnings announcement date; (4) it has non-missing earnings numbers on both Preliminary and AFR Compustat quarterly databases (IBQ_P and IBQR); (5) it has non-missing net operating cash flow on the AFR Compustat quarterly database (OANCFQR); (6) it have non-missing total assets at the end of the prior quarter (lag_ATQR); (7) its book value of equity at prior quarter-end (lag_SEQQR) is positive; (8) the SEC filing date can be found in the Standard & Poor's SEC filing date database and the SEC filing date is at least seven days after the earnings announcement date; (9) the cumulative return during the three-day window centered on the preliminary earnings announcement date and on the SEC filing date can be computed from CRSP; (10) the cumulative return during the pre-event window and the post-event window can be calculated (pre-event window is defined as the [−4, −2] days relative to SEC filing date, post-event window is defined as the [+6, +8] days relative to SEC filing date); (11) it has valid current and lagged (i.e., the same fiscal quarter in the previous fiscal year) observations on all the variables employed in the VAR system (i.e., event, pre-event, and post-event cumulative returns, ROE and BM around the preliminary earnings announcement date and SEC filing date, and accruals and net operating cash flows around the filing date); (12) any of the variables employed in the VAR system is not in the top or bottom 1 percentile of the distribution; (13) the absolute value of the difference between income before extraordinary items at the filing date (IBQR) and preliminary date (IBQ_P) is less than 1 percent of the absolute value of income before extraordinary items at the preliminary date.

[b] This table presents the market return variance decomposition results for the periodic SEC filing using a four-element VAR model. The four elements include log of three-day cumulative return (r_t), cash flow from operations scaled by beginning of period book value of equity (CF_t), accruals scaled by beginning of period book value of equity (ACC_t), and the log book-to-market ratio (bm_t). The VAR model is $z_t = Az_{t-1} + \eta_t$, where $z_t = (r_t, CF_t, ACC_t, bm_t)'$ and $\Omega = E(\eta_t, \eta_t')$. The VAR model is estimated using panel data with firm and fiscal quarter fixed effects. The VAR model is estimated separately for three different three-day windows: Pre-Event Days ([−4, −2]), Event Days ([−1, +1]), and Post-Event Days ([+6, +8]) (all being relative to the periodic SEC filing date). *VARNRF* is the variance of expected-return news at the three-day window calculated as $\lambda_1'\Omega\lambda_1$, where $\lambda_1' = e_1'\rho A(I - \rho A)^{-1}$, $e_1' = [1, 0, 0, 0]$ and $\rho = 0.99$. *VARNCFF* is the variance of cash flow news at the three-day window calculated as $\lambda_2'\Omega\lambda_2$, where $\lambda_2' = e_2'(I - \rho A)^{-1}$ and $e_2' = [0, 1, 0, 0]$. *VARNACF* is the variance of accrual news at the three-day window calculated as $\lambda_3'\Omega\lambda_3$, where $\lambda_3' = e_3'(I - \rho A)^{-1}$ and $e_3' = [0, 0, 1, 0]$. In the Regression Results for Periodic SEC Filing Date Cash Flow-Accrual Model

(*continued on next page*)

TABLE 8 (continued)

Variance Decomposition, dependent variables are the three variance components. *EVENT* takes the value of 1 if it is the Event Day [−1,+1] relative to periodic SEC filing date, and 0 otherwise. *OVERDUE* takes the value of 1 if either of the following conditions is met: (1) 10-K filing date ≥ 92 calendar days from the fiscal year-end and if the fiscal year ended before December 15, 2003 or the firm is not an Accelerated Filer; (2) 10-K filing date ≥ 77 calendar days from the fiscal year-end if the fiscal year of an Accelerated Filer ended between December 15, 2003 and December 15, 2006; (3) 10-Q filing date ≥ 47 calendar days from the fiscal quarter-end and if the immediately previous fiscal year ended before December 15, 2003 or the firm is not an Accelerated Filer; (4) 10-Q filing date ≥ 42 calendar days from the fiscal quarter-end if the immediately previous fiscal year of an Accelerated Filer ended between December 15, 2003 and December 15, 2006, and 0 otherwise. *NT* takes the value of 1 if it is an overdue filing and an NT 10-K (NT 10-Q) has been filed between the preliminary earnings announcement date and the 10-K (10-Q) filing date, and 0 otherwise.

VII. CONCLUDING REMARKS

This study explores the circumstances in which a significant market reaction around periodic SEC filings is observed in the EDGAR era. Specifically, the paper examines the extent to which concurrent earnings releases and the calendar-timing clustering in firms' filings of periodic reports impact tests of market reaction surrounding periodic SEC filings. The sizeable incidence of concurrent earnings releases provides us the impetus to re-examine the economic and statistical significance of market reaction to annual and interim periodic filings.

Further, our analysis provides new evidence regarding circumstances in which a market reaction is observed surrounding the filing of periodic reports. We find significant price and volume reactions when periodic reports include earnings information for the first time. However, with the exception of 10-K reports, we find no evidence of significant market reactions for the most common case of periodic reports that follow earnings announcements.

To better understand why 10-K reports are an exception, we explore several possible reasons for why the result for the 10-K reports could be due to the high incidence of their clustering around calendar quarter-ends. First, we document that the market reaction to 10-K reports is limited to those filed around calendar quarter-ends, which account for less than one quarter of all 10-K reports. Consistent with the turn-of-the-quarter effect documented in the finance literature, we find a calendar quarter-end effect in both price and volume reactions unrelated to the filing 10-K reports. However, while there is some evidence of an incremental price reaction to 10-K reports over and above the calendar quarter-end effect, the trading volume around calendar quarter-ends is indistinguishable between filers and non-filers. Second, we eliminate self-selection as a possible explanation because firms that typically file around non-calendar quarter-ends experience higher market reactions when they uncharacteristically change the calendar-time of their filings. Third, we find at least partial support for the conjecture that the calendar-quarter effect at the market level represents an information-transfer effect due to the stock prices and trading volumes of non-filing firms reacting to information in periodic reports released by filers from the same industry. Finally, we find that equity analyst reactions are muted around periodic filings, and there is no evidence of calendar-time clustering in analyst reactions, suggesting that analysts are not a mechanism for the information transfer.

Several caveats are in order. First, with respect to competing explanations for the quarter-end effect, our study considers intra-industry information transfer due to the intensity of information arrival surrounding calendar quarters and the forecasting activity of equity analysts. Future research could examine other explanations for the heightened price and volume reactions during times in which the market is inundated with corporate disclosures. Second, market participants may react to periodic SEC filings in which the magnitude of the market activity may be difficult to detect in large-sample market reaction tests. Third, while our study finds no pervasive evidence of market reaction to periodic SEC filings (especially the 10-Q filings), we are not suggesting that SEC filings have no economic or informational value. Rather, the demand for the services of data aggregators such as Standard & Poor's, FactSet, and Bloomberg indicate that various market participants such as money managers, other institutional investors, and credit analysts must consider the information in periodic SEC filings collected by data aggregators to be valuable for sophisticated investment and other economic decisions.[25] Thus, future research could explore how information in periodic reports is collected and disseminated by data aggregators, and how it is used by different market participants.

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[25] Focusing on a different group of market participants, Balsam et al. (2002) find that unsophisticated investors undo the effects of earnings management (proxied by large unexpected discretionary accruals) during a three-week period following the release of a 10-Q. However, they find statistically significant price changes consistent with the discovery of earnings management only after five trading days following the 10-Q release.

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Fraud Brainstorming Using Computer-Mediated Communication: The Effects of Brainstorming Technique and Facilitation

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ABSTRACT: This study experimentally investigates the effectiveness of computer-mediated brainstorming in the context of the SAS No. 99 mandated fraud brainstorming requirement. Results indicate that brainstorming effectiveness is significantly higher for teams brainstorming electronically relative to teams using traditional face-to-face brainstorming. There is no significant difference in brainstorming effectiveness between electronic interactive brainstorming and electronic nominal brainstorming. Brainstorming effectiveness is significantly higher for teams receiving content facilitation relative to teams not receiving content facilitation. The post-brainstorming fraud risk assessments are significantly higher than the pre-brainstorming assessments in all treatment conditions, indicating that the SAS No. 99 mandated brainstorming session has the intended effect. This research informs audit practice by demonstrating that computer-mediated fraud brainstorming can be more effective than face-to-face brainstorming and also by establishing the effectiveness of content facilitation for improving the quality of the fraud risk factors generated by auditors.

Keywords: *fraud; group support systems; interactive brainstorming; nominal brainstorming; automated content facilitation; audit teams.*

Data Availability: *Contact the first author for data. Contact the second author for details regarding the electronic fraud brainstorming system.*

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I. INTRODUCTION

In the wake of well-publicized corporate scandals, the American Institute of Certified Public Accountants (AICPA) revised its existing fraud guidance by promulgating Statement on Auditing Standards (SAS) No. 99, *Consideration of Fraud in a Financial Statement Audit* (AICPA 2002). This new standard, which became effective for financial statement audits covering periods beginning on or after December 15, 2002[1] requires auditors to conduct a fraud brainstorming session(s) during audit planning. The overall objectives of fraud brainstorming are to identify fraud risk factors associated with the client and to emphasize to all members of the audit team the importance of a "questioning mind" and "professional skepticism" related to fraud. SAS No. 99 does not prescribe specific types of brainstorming formats, and it is very common for audit teams[2] to conduct their fraud brainstorming sessions in a face-to-face multiperson setting (Brazel et al. 2007).

In a multiperson audit decision-making setting, such as fraud brainstorming, auditors interact to share ideas, discuss alternatives, and reach consensus regarding decisions that need to be made. The benefits of making decisions and performing certain tasks within a multiperson setting have been well documented in prior research in contexts other than fraud brainstorming (Schultz and Reckers 1981; Johnson 1994; Hirst and Koonce 1996; Bedard and Maroney 2000). Auditing research has only recently begun to investigate multiperson judgment- and decision-making issues in the context of fraud brainstorming (Lynch 2006; Carpenter 2007). To meet the requirements of SAS No. 99, fraud brainstorming can be accomplished in many ways, including meeting face-to-face or in a computer-mediated environment. Tools for computer-mediated communication, known variously as electronic meeting systems or group support systems (GSS), provide a structured environment and can overcome time and space boundaries, allowing members of a team to collaboratively generate ideas, make decisions, and solve problems (Jessup et al. 1990). Although accounting research has examined the effects of GSS on various aspects of audit team decision-making (Kerr and Murthy 1994; Bamber et al. 1996; Karan et al. 1996; Ho 1999; Murthy and Kerr 2004), the potential effects of GSS on fraud brainstorming in a SAS No. 99 context have not been investigated.

In a face-to-face brainstorming team, individuals must wait for their turn to speak, and social norms dictate that members must attend to the person speaking. These characteristics of face-to-face meetings result in production blocking—a cognitive interference mechanism that inhibits idea-generation productivity (Diehl and Stroebe 1987; Mullen et al. 1991; Nijstad, Diehl, and Stroebe 2003; Nijstad, Stroebe, and Lodewijkx 2003). For a brainstorming team using computer-mediated communication ("electronic brainstorming"), production blocking is mitigated since members can input their ideas simultaneously. A considerable body of research in information systems (IS) has established that electronic brainstorming is superior to face-to-face brainstorming for simple idea-generation tasks (Gallupe et al. 1991; Nunamaker et al. 1991; Fjermestad and Hiltz 1998). Fraud brainstorming, however, is more complex than simple idea-generation tasks in that participants must draw on specialized knowledge of auditing and client-specific factors to identify *relevant* fraud risks. Accordingly, it is unclear whether the results from the IS literature hold in the context of fraud brainstorming, since the fraud brainstorming task has elements of a cognitive conflict task for which groups interacting face-to-face have generally outperformed groups using computer-mediated communication (Fjermestad and Hiltz 1998).

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[1] The Public Company Accounting Oversight Board (PCAOB) subsequently adopted SAS No. 99 as part of its Interim Professional Auditing Standards.

[2] While SAS No. 99 permits the exercise of professional judgment in the selection of brainstorming participants, key members of the audit team of all experience levels must be included. It should be noted that SAS No. 99 allows audit teams as small as one member, as long as the previously mentioned brainstorming objectives are met.

Computer-mediated communication can facilitate brainstorming in one of two primary ways. Interactive brainstorming involves members who input their own ideas and can see the ideas of other team members in real-time. Alternatively, nominal brainstorming describes a technique in which members of the team input ideas individually with no interaction with the rest of the team, following which all individually generated ideas are merged and available for the team to view (Dennis and Reinicke 2004). The relative effectiveness of electronic interactive and nominal brainstorming is important to investigate because interactive brainstorming closely parallels commonly employed SAS No. 99 fraud brainstorming applications while the nominal technique does not require participants to brainstorm concurrently, with concomitant logistical efficiencies. Another factor that can affect the effectiveness of brainstorming is the provision of content-specific prompts to facilitate the generation of relevant ideas during the brainstorming session. Relying on Spreading Activation Theory (Collins and Loftus 1975), we expect that content facilitation will stimulate the retrieval of prompt-related information from memory, leading to superior brainstorming performance.

The purpose of this study is to investigate, in the context of the SAS No. 99 mandated fraud brainstorming session, (1) the relative effectiveness of electronic versus face-to-face brainstorming, (2) the relative effectiveness of two forms of electronic brainstorming (i.e., interactive versus nominal team brainstorming), (3) whether content facilitation improves brainstorming effectiveness, and (4) whether participating in a fraud brainstorming session heightens auditors' awareness of fraud risks when warranted, i.e., when significant fraud risks are present in the auditee's environment. We design a 3 × 2 (brainstorming mode × facilitation) between-subjects experiment using auditing student participants to test the research hypotheses. Results reveal that brainstorming effectiveness, defined as the number of relevant fraud risk factors identified, is significantly higher for groups brainstorming electronically relative to groups brainstorming face-to-face. There is no significant difference in brainstorming effectiveness between electronic interactive brainstorming and electronic nominal brainstorming. In both electronic and face-to-face brainstorming conditions, brainstorming effectiveness is significantly higher for teams receiving content facilitation relative to teams not receiving content facilitation. Finally, in all treatment conditions, the post-brainstorming fraud risk assessment is significantly higher than the initial fraud risk assessment. In the aggregate, the results contribute to the accounting literature and inform audit practice by demonstrating that computer-mediated fraud brainstorming can be superior to face-to-face brainstorming and also by establishing the effectiveness of content facilitation for improving the quantity of fraud risk factors identified by auditors.

In Section II, we review relevant literature and develop hypotheses. In Section III, we describe the research method. Sections IV and V present the results and discuss the implications of the study, respectively.

II. BACKGROUND AND HYPOTHESES

SAS No. 99 and Brainstorming Techniques

The objective of SAS No. 99 is to enhance the likelihood that audit teams will uncover material fraud affecting the financial statements during the course of an audit. To fulfill the requirements of SAS No. 99, the audit team must generate ideas during the audit-planning stage about the possibility of fraudulent financial reporting and misappropriation of assets in the context of the client to be audited. The purpose of this fraud brainstorming session is twofold: (1) to enable the audit team to share ideas regarding how fraud might be perpetrated in the environment of the upcoming audit; and (2) to sensitize the audit team to the possibility of fraud, so that team members will approach the audit with the appropriate level of professional skepticism. The expectation is that a thorough discussion about the client's susceptibility to fraud will heighten auditors' awareness that the possibility of fraud exists in every audit engagement, regardless of their prior experiences with the client and *prima facie* appearances of management's honesty and integrity.

SAS No. 99 is silent on the format of the required brainstorming session. A face-to-face meeting is the most natural configuration for the session and is the format most commonly used (Brazel et al. 2007). In a face-to-face brainstorming session, however, there can be only one person speaking at a time, participants must pay attention to the person speaking, and the time taken by each speaker can vary considerably. These inexorable aspects of face-to-face meetings result in the production-blocking phenomenon, which is manifested in delays between the generation of ideas in memory and the articulation of those ideas (Nijstad, Stroebe, and Lodewijkx 2003). Nijstad, Stroebe, and Lodewijkx's (2003) two-stage theory of production blocking identifies two impediments to idea generation in face-to-face brainstorming—delay length and delay predictability. The longer the delay (typically caused by an individual speaking for a long time), the lower the likelihood that ideas will be verbalized because the train of thought of those listening is disrupted. The lack of delay predictability stems from uncertainty regarding when one's turn to speak might come, decreasing the likelihood that ideas that occur to an individual can be immediately verbalized.

Communicating electronically eliminates or minimizes most of the process losses associated with face-to-face communication, in particular production blocking.[3] In a computer-mediated brainstorming session, each participant interacts via a computer and all participants can input their ideas simultaneously. In terms of Nijstad, Stroebe, and Lodewijkx's (2003) two-stage theory of production blocking, simultaneous idea input eliminates delay length and delay predictability as inhibitors of the cognitive processes involved in brainstorming. SAS No. 99 does not preclude the use of the computer to facilitate the brainstorming session. Our first objective in this study is to investigate whether electronic fraud brainstorming is superior to face-to-face fraud brainstorming.

Face-to-Face versus Electronic Brainstorming

No prior research has compared electronic fraud brainstorming to face-to-face fraud brainstorming. There is evidence in the accounting literature, however, of superior outcomes resulting from face-to-face interaction relative to electronic interaction. In an audit review setting, Brazel et al. (2004) find that auditors anticipating a face-to-face review are more effective and less efficient than those anticipating an electronic review. They also find a higher sense of accountability for preparers in the face-to-face review condition. The findings from Brazel et al. (2004) indicate that, in an audit review setting, face-to-face communication may be superior to electronic communication. Results would likely differ, however, in a fraud brainstorming setting. In the Brazel et al. (2004) study the focus was on individual auditor performance in workpaper preparation and going-concern evaluation within an audit review setting. In fraud brainstorming, the focus is on audit team performance where the goal is to jointly produce a list of relevant fraud risk factors. In addition, the audit review setting involves interaction between auditors with differing task objectives—one prepares the workpapers and the other evaluates those workpapers. In a fraud brainstorming task, all participating auditors have the same task objective of generating relevant fraud risk factors.

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[3] Bamber et al. (1998) and Murthy (2002) provide comprehensive discussions of the relative process losses and gains in comparing face-to-face and computer-mediated team settings.

There is considerable evidence in extant IS literature that electronic brainstorming is superior to face-to-face brainstorming for simple idea-generation tasks (Connolly et al. 1990; Jessup et al. 1990; Gallupe et al. 1991; Dennis and Valacich 1993; Valacich et al. 1994). In the typical IS electronic brainstorming study using a simple idea-generation task, participants brainstorm about issues such as how the parking problem on campus can be solved or how tourism in their city can be improved. There are no "right" and "wrong" answers in such tasks, nor do they require participants to draw on much domain-specific knowledge. By contrast, the SAS No. 99 mandated fraud brainstorming task is more complex, in that participants (i.e., auditors) must draw on knowledge of accounting regulations, client-specific factors, and industry-specific factors to identify *relevant* fraud risk factors that may or may not be present in the audit client's environment. In terms of McGrath's (1984) "task circumplex," a widely recognized taxonomy of group tasks, the simple idea-generation tasks used in the IS electronic brainstorming research fall in Quadrant I. Cognitive conflict tasks, which fall in Quadrant III of McGrath's task circumplex, are those that involve mentally resolving conflicting viewpoints. In a fraud brainstorming session conducted during the audit-planning stage, the cognitive conflict arises from the uncertainty in deciding whether a possible fraud risk is relevant for the current audit. Accordingly, although a fraud brainstorming task has elements of idea-generation tasks (i.e., maximizing the number of fraud risks identified), it also has elements of cognitive conflict tasks (i.e., distinguishing between *relevant* and *irrelevant* fraud risks).

Fjermestad and Hiltz (1998) present a comprehensive review of the results from over 200 experiments investigating the effects of computer-based support for decision making. Summarizing the results by task type, Fjermestad and Hiltz (1998) indicate that when group support systems are used for idea-generation tasks, teams using computer-mediated communication generally outperform teams interacting face-to-face. For cognitive conflict tasks, however, teams interacting face-to-face generally outperform teams using computer-mediated communication. Since the fraud brainstorming task has elements of both idea-generation tasks and cognitive conflict tasks, the results from the IS literature cannot be assumed to hold for the fraud brainstorming setting. In this study, we investigate whether computer-mediated communication results in superior performance compared to face-to-face communication for the SAS No. 99 fraud brainstorming session. While the cognitive conflict aspect of the fraud brainstorming task and the findings from prior accounting studies in contexts other than brainstorming (e.g., Brazel et al. 2004) provide empirical tension, given the mitigation of production blocking accruing from computer-mediated communication and the evidence from the IS literature in support of electronic brainstorming being more effective than face-to-face brainstorming, we test the following hypothesis:

> **H1:** Teams brainstorming electronically will generate a greater number of relevant fraud risks than teams brainstorming face-to-face.

Interactive versus Nominal Brainstorming

The most common implementation of electronic brainstorming is interactive brainstorming, in which team members simultaneously input and exchange ideas with each other and can build off the ideas of others.[4] Alternatively, in the *nominal group technique* (Delbecq and Van de Ven 1971) individuals are part of a team, but brainstorm alone without the input of other team members. Later, their ideas are merged with those of other team members who also brainstormed alone (Dennis and Reinicke 2004). The merged set of ideas, after eliminating duplicates, constitutes the total number of unique ideas developed by the team. From a procedural standpoint, it is worth noting that a significant advantage of the nominal group technique is that members do not have to engage in the brainstorming process concurrently—they can input their ideas at different times, eliminating the need to coordinate the schedules of brainstorming participants. It is this feature of nominal group brainstorming that primarily justifies a comparison between nominal and interactive brainstorming.

In comparing electronic nominal brainstorming to electronic interactive brainstorming, there are advantages and disadvantages inherent in each technique. One problem that can arise in interacting teams is cognitive inertia—the inability of team members to think outside the frame of ideas previously submitted (Dennis et al. 1997). In other words, team members tend to suggest ideas similar or consistent with previously submitted ideas, resulting in a narrower range of ideas. A second disadvantage associated with electronic interactive brainstorming and not electronic nominal brainstorming is distraction conflict (Niederman et al. 1996; Aiken et al. 1997; Pinsonneault et al. 1999; Potter and Balthazard 2004). For example, Potter and Balthazard (2004) find that sources of distraction such as off-task comments made by others can inhibit team members' ability to generate on-task relevant ideas. Additionally, individuals can become distracted as they read others' ideas and comments, which can inhibit the number of ideas they submit themselves.

Individuals who brainstorm in a nominal group are not distracted by others' ideas. Nominal group brainstorming makes cognitive inertia and pitfalls such as groupthink less likely because each member generates ideas individually. Members are not pressured to conform to the thought processes of other members, allowing individuals to focus on their own points of view without facing opposition. This uninhibited flow of thought without interruption has been linked to individual freedom to express, a necessary basic principle for brainstorming (Goncalo and Staw 2006). Brainstorming alone, however, makes it impossible to piggyback on the ideas of others. An individual brainstorming as part of a nominal group is not exposed to the ideas of others, and thus lacks stimuli from other group members, which can be especially problematic when a member is in a state of "mental block." In light of the advantages and disadvantages of both techniques, it is not surprising that prior research has found no significant difference between the techniques in applications other than fraud brainstorming (Gallupe et al. 1991; Valacich et al. 1994). Given the approximately equal number of advantages and disadvantages associated with electronic interactive and electronic nominal brainstorming, we expect that either technique could be associated with greater brainstorming effectiveness. Accordingly, we test the following null hypothesis against a two-tailed alternative:

> **H2:** There will be no difference in the number of relevant fraud risks identified between audit teams using the electronic nominal brainstorming technique and audit teams using the electronic interactive brainstorming technique.

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[4] See Dennis and Reinicke (2004) for a detailed description of alternative electronic brainstorming methods.

Facilitation

Prior research suggests that most ideas in a brainstorming session are generated during the early stages of the session (Diehl and Stroebe 1991; Nijstad and Stroebe 2006). In essence, brainstorming participants can be mired in a state of cognitive inertia after a period of time and may have difficulty generating new ideas. It has been proposed that exposure to stimuli such as prompts can remove writer's block and spur idea generation (Hender et al. 2002; Santanen et al. 2004). According to Spreading Activation Theory (Collins and Loftus 1975), the activation of specific areas of memory can lead to the activation of related nodes in memory via a spreading process. Prompts (cues) to brainstorming participants can trigger this spreading-activation process by specifically invoking certain memory nodes, leading to the activation of related nodes and ultimately spurring idea generation. As Santanen et al. (2004, 177) suggest, "context-relevant stimuli (such as hints provided by a facilitator) may help the problem solver activate frames that may not lie in the direct path of automatic spreading activation and explore vastly different areas of their knowledge network, thereby avoiding the experience of being 'stuck in a rut.'"

To trigger the spreading-activation process, a facilitator can deliver prompts to participants in a face-to-face brainstorming session. Per Bostrom et al. (1993, 147), facilitation comprises the "set of functions or activities carried out before, during, and after a meeting to help the group achieve its own outcomes." Providing the group with specific alternatives to consider as they brainstorm in an attempt to influence the substance of the group's discussions is termed *content* facilitation (Bostrom et al. 1993).[5] In a computer-mediated brainstorming session, content facilitation can be automated, such that the system prompts participants on the computer screen at predetermined intervals.

Content facilitation, by way of suggestions for audit team members to consider, can be thought of as a form of decision aid. Prior auditing research suggests that most auditors, especially novice auditors, are not experts at identifying and detecting fraud risk, such that the use of some form of guidance or decision aid can assist them in their fraud risk assessment (Eining et al. 1997; Wilks and Zimbelman 2004). Eining et al. (1997) find that auditors using a fraud risk assessment decision aid, in the form of an expert system designed to engage the user and increase reliance, made better decisions than did auditors using a decision aid that provided only a suggested assessment. Wilks and Zimbelman (2004) argue that fraud checklists, a form of decision aid, can be structured to facilitate strategic reasoning and increase the accuracy of fraud risk judgments. There is, however, evidence in the auditing literature that providing decision makers with support by way of decision aids or checklists can be detrimental to performance. Pincus (1989) finds that providing auditors with a "red flags" checklist does not result in improved assessments of management fraud, while Asare and Wright (2004) find that auditors using a standard fraud risk checklist make lower fraud risk assessments than those with no checklist. These results suggest that decision aids do not always improve auditor performance, underscoring the need for research on whether decision-aiding mechanisms can help in the arena of the SAS No. 99 mandated fraud brainstorming session.

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[5] A second type of facilitation is *process* facilitation, which could involve setting the meeting agenda, determining and controlling which group member participates and when that member's participation begins and ends, and moving the group through the various stages of a multi-step task. In virtually all GSS facilitation studies, the content or process facilitation is provided by a human facilitator. With human facilitation, it is difficult to discern the extent to which the observed effects are a function of the specific individuals providing the facilitation relative to the process or content facilitation actually being provided. In a computer-mediated brainstorming session, however, it is possible to automate content facilitation by configuring the GSS to automatically provide participants with stimuli as they brainstorm. The advantage of such automated facilitation is that any observed effects can be ascribed to the content itself without the confounding effects of individual facilitation style as is the case with a human facilitator.

We investigate the effectiveness of providing content facilitation by way of fraud prompts for teams brainstorming face-to-face and teams using computer-mediated brainstorming. Drawing on Collins and Loftus' (1975) Spreading Activation Theory of memory activation in response to prompts and given prior auditing research on the benefits of decision aids, we expect that content facilitation will enhance fraud brainstorming productivity, leading to the following hypothesis:

> **H3:** Teams receiving content facilitation will generate a greater number of relevant fraud risks than teams not receiving content facilitation.

Effect of Brainstorming on Fraud Risk Assessments

One of the main reasons for the SAS No. 99 requirement of fraud brainstorming in the early stages of the audit is to heighten auditors' sensitivity to risk factors indicative of fraud as they proceed with audit fieldwork. In order to ensure that team members, especially relatively inexperienced team members such as staff auditors, maintain professional skepticism throughout the audit and do not dismiss relevant evidence, an objective of the brainstorming session is to maximize audit team members' exposure to, and awareness of, the various fraud risk factors that the team identifies. For all participants, especially junior, less experienced team members, the fraud brainstorming session serves as a form of training. Goldstein (1991, 508) defines the training process as "the systematic acquisition of attitudes, concepts, knowledge, rules, or skills that result in improved performance at work." Given that each member of the audit team brings a separate set of skills, knowledge, and experience, a systematic exchange of ideas among members should lead to improved performance and expertise in evaluating evidence in a similar or different setting (Goldstein 1991).

The fraud brainstorming process can be considered a form of "cooperative learning," a pedagogical technique for which there is considerable support. Cooperative learning has been defined as the use of small groups in instructional settings, allowing students to work together to maximize their own and each other's learning (Johnson and Johnson 2005). Research on cooperative learning has produced evidence that there often is significant knowledge transfer from the group to the individual participants. Examples of research providing evidence of knowledge transfer from groups to the participating individuals include Paulus and Yang (2000) and Olivera and Straus (2004).

Paulus and Yang (2000) find evidence of group-to-individual transfer with an idea-generation task involving "brainwriting" (i.e., the sharing of written ideas in groups). The experiment had the subjects generate ideas for possible uses of paper clips. This task was similar to the generation of significant fraud risks in a SAS No. 99 fraud brainstorming session, in that the "correct answer" is not completely discernable. Olivera and Straus (2004) also investigate the effect of group interaction on subsequent individual performance. Their hypotheses are based on the models of Hackman and Morris (1975) and McGrath (1984) and a large body of research involving cooperative learning.[6] The Olivera and Straus (2004) research involves experiments utilizing a "brainteaser puzzle," an intellective task in which correct answers are discernable. The researchers find that performance is enhanced in a similar subsequent task for individuals who interact with other group members in a previous task relative to those who act alone in a previous task.

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[6] Theories supporting the effectiveness of cooperative learning have been categorized as motivational or cognitive. For a more detailed explanation, refer to Slavin (1995).

Taken collectively, there is a large body of cooperative learning research suggesting that knowledge transfer would occur from a fraud brainstorming team to the individual auditors. For audits in which significant fraud risk factors are present, auditors will very likely become aware of fraud risk factors during the brainstorming session that they had not thought of prior to the session. From an audit effectiveness perspective, in a scenario where significant fraud risks are present, auditors' judgments should reflect the increased risk so that the ensuing audit plan can be appropriately modified. Accordingly, we posit that the increased awareness of fraud risk factors from participating in the fraud brainstorming session should be reflected in higher post-brainstorming fraud risk assessments. Thus, we test the following hypothesis:

> **H4:** After engaging in a fraud brainstorming session in a scenario where significant fraud risks are present, auditors' subsequent fraud risk assessments will be greater relative to their fraud risk assessments prior to brainstorming.

In sum, we draw on relevant prior research and theory to test four hypotheses: (1) teams brainstorming electronically will outperform teams brainstorming face-to-face (H1); (2) a two-tailed test against the null hypothesis of no difference in brainstorming effectiveness between teams using the electronic nominal brainstorming technique and teams using the electronic interactive brainstorming technique (H2); (3) content facilitation will improve brainstorming effectiveness (H3); and (4) the fraud brainstorming session in a scenario where significant fraud risks are present will heighten auditors' sensitivity to fraud risk as reflected in post-brainstorming fraud risk assessments (H4). The next section presents the research method.

III. METHOD

To test the research hypotheses, we employ a 3 (brainstorming mode) × 2 (facilitation) between-subjects experiment. The three levels of the brainstorming mode factor are: face-to-face, electronic interactive, or electronic nominal brainstorming. The two levels of the facilitation factor manipulate the provision or absence of content facilitation. The research design and experimental procedures are illustrated in Figure 1.

Participants

A total of 188 auditing students participated in the experiment.[7] Peecher and Solomon (2001) contend that the use of students instead of experienced auditors as participants in an experiment is inappropriate only if theory or prior research provide a basis for expecting experience to interact with a factor of interest in the study, thereby causing a threat to internal validity.[8] In our study, the focus is on the relative differences in fraud brainstorming effectiveness as a function of the mode of brainstorming (face-to-face or computer-mediated) and content facilitation (provided or not provided). Regarding the brainstorming

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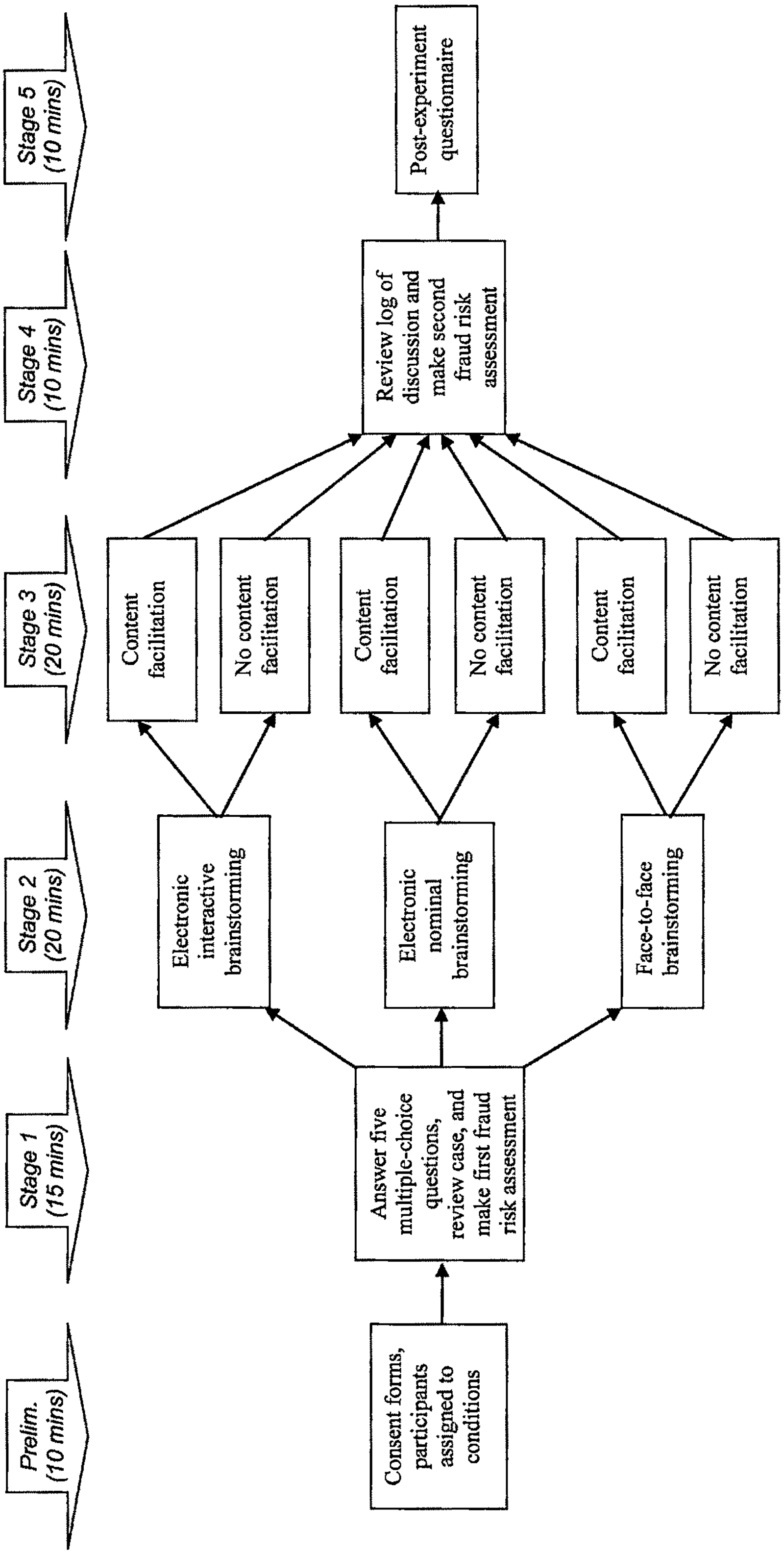
[7] The student participants were drawn from multiple undergraduate auditing classes and one auditing class in a Master's of Accountancy program. All students received similar instruction in SAS No. 99 requirements.

[8] Per Peecher and Solomon (2001), in the presence of an interaction between experience and an independent variable in the study, any effects observed cannot reliably be attributed to the manipulated independent variable. In essence, the interaction, if present, would indicate that the effects observed using student participants would not hold if the study were replicated using experienced auditors as participants.

FIGURE 1
Research Design and Five Stages of the Experimental Procedure


Prelim. (10 mins)
Stage 1 (15 mins)
Stage 2 (20 mins)
Stage 3 (20 mins)
Stage 4 (10 mins)
Stage 5 (10 mins)
Consent forms, participants assigned to conditions
Answer five multiple-choice questions, review case, and make first fraud risk assessment
Electronic interactive brainstorming
Electronic nominal brainstorming
Face-to-face brainstorming
Content facilitation
No content facilitation
Content facilitation
No content facilitation
Content facilitation
No content facilitation
Review log of discussion and make second fraud risk assessment
Post-experiment questionnaire

mode factor, the theoretical basis for expecting computer-mediated brainstorming teams to outperform face-to-face teams involves the process losses inherent in face-to-face communication, primarily the production-blocking phenomenon. Paulus et al. (1995) investigated whether the productivity losses in face-to-face brainstorming found in laboratory settings using student participants persist in organizational settings, in which professionals who have worked together brainstorm on issues of relevance to them and their organization. They conclude that the productivity losses in face-to-face brainstorming occur "even in groups who work together on a daily basis, have considerable training in group dynamics, and are dealing with a job-relevant issue" (Paulus et al. 1995, 249). Thus, experience is unlikely to interact with the first factor of interest in our study, i.e., brainstorming mode.

For the content facilitation factor, our expectations are based on Spreading Activation Theory (Collins and Loftus 1975), which posits that prompts should facilitate the activation of related items in memory. An interaction between facilitation and audit experience that would pose a validity threat for the current study is if experienced auditors respond negatively (or not at all) to facilitation prompts. To the contrary, research by Moeckel (1990) indicates that the spreading-activation effect is more pronounced for experienced than inexperienced auditors. Similarly, Libby and Frederick (1990) report that experienced auditors reacted more positively than students to prompts in a hypothesis-generation task. Based on these prior research findings, we conclude that experience is unlikely to interact with either brainstorming mode or content facilitation in a manner that would invalidate conclusions about enhanced brainstorming effectiveness stemming from these factors. Therefore, for the reasons indicated above, the use of students as participants does not appear to threaten internal validity.

For all participants, the topics of SAS No. 99 and fraud brainstorming were covered in class prior to the experiment. In teams brainstorming electronically, 108 individuals participated; and in teams brainstorming face-to-face, 80 individuals participated. Participants were grouped by quartile based on their grade ranking in the auditing class at midterm, with the bottom quartile being assigned the role of staff, the second quartile assigned the role of senior, the third quartile assigned the role of manager, and the top quartile assigned the role of partner. This role assignment was intended to instill realism by simulating a practice scenario in which audit teams would comprise a mix of higher knowledge/ability (partner, manager) and lower knowledge/ability (senior, staff) auditors. The participants were instructed to prepare for the experiment by studying a case. To test if knowledge of the case was significantly different among assigned roles, participants were asked five multiple-choice questions about the assigned case prior to brainstorming. Each correct answer was worth one point. Univariate analyses revealed that those participants assigned the role of staff auditor scored lower on the multiple choice questions than all other ranks ($F = 2.687$, $p < 0.05$). On average, students assigned the role of partners scored 4.57, those assigned the role of managers scored 4.47, those assigned the role of seniors scored 4.43, and those assigned the role of staff auditors scored 4.13 out of a maximum of 5 points. Further, in teams brainstorming electronically,[9] participants classified as either a partner or manager generated more relevant fraud risk factors ($\bar{x} = 7.89$), on average, than senior and staff participants combined ($\bar{x} = 6.85$) ($p < 0.10$, one-tailed). These statistical results support the notion that the audit teams organized for the purpose of the experiment had a mix of higher and lower knowledge/ability participants, consistent with what would be found in real-world audit teams. Table 1 shows demographics on study participants.[10]

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[9] Participant rank was identifiable in electronic brainstorming groups, since each individual idea was recorded by the system. In groups brainstorming face-to-face, the scribe-generated list of ideas did not identify the participant who suggested the idea. Consequently, the analysis of ideas by participant rank is performed only for groups brainstorming electronically.

Task

One week prior to the experiment, participants were provided a copy of the Helecom Communications case developed by Ballou and Mueller (2005a). This case contains a detailed and realistic description of a client organization that had numerous fraud risk factors. The case was developed specifically for undergraduate and graduate accounting students, and includes a model solution of the fraud risk factors present. Having participants study the case prior to the brainstorming session is a procedure recommended by Beasley and Jenkins (2003), analogous to the professional audit requirement of having auditors develop an understanding of the client and the client's industry. At the beginning of the experiment, participants were required to answer five multiple-choice questions designed to assess their level of knowledge regarding the Helecom Communications case. The mean number of correctly answered questions for participants in the electronic brainstorming treatments is 4.40, while the mean score for participants in the face-to-face brainstorming treatments is 4.48. In both situations, at least 80 percent of the participants responded correctly to four of the five questions, indicating that participants had conscientiously completed the advance reading in preparation for the brainstorming session. Additionally, univariate analysis of the mean difference in multiple-choice scores across treatments is insignificant ($F = .80$; $p = .452$). These results provide support for the position that case-material knowledge (case preparation) is randomly distributed across treatments.

Experimental Procedures

Participants were assigned to roles as indicated earlier and four-person teams were formed such that each team was comprised of a partner, a manager, a senior, and a staff auditor. Each team was then randomly assigned to a treatment condition (electronic interactive brainstorming, electronic nominal brainstorming, or face-to-face brainstorming, with or without content facilitation). Participants in teams assigned to one of the electronic brainstorming treatments then logged on to a computer application designed for the purpose of the experiment. Participants in teams assigned to one of the face-to-face treatments brainstormed in a private room that included a laptop, voice recorder, and name/rank tents for each participant. Participants in face-to-face teams completed experimental elements such as answering the multiple-choice questions on paper. As each face-to-face team brainstormed by verbally discussing the case, fraud risk factors were typed using the laptop by one of the team members. Each session was tape recorded.

The experiment consisted of five stages (see Figure 1). In Stage 1, participants answered the five multiple-choice questions previously described, were given time to briefly review

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[10] Using Chi-square analyses, we compare demographics across experimental conditions for class standing, age, gender, and exposure to SAS No. 99. With the exception of class standing, there are no statistically significant differences in participant demographics across treatments. Participants' class standing (junior, senior, 5th year/graduate) is unevenly distributed among experimental conditions. Specifically, the number of 5th year/graduate students is significantly higher in the electronic brainstorming conditions relative to the face-to-face conditions. This was the result of one class of 24 master's degree students (six teams) being exclusively assigned to electronic brainstorming conditions. To assess the potential impact of this difference, we re-ran the statistics after excluding all 24 master's degree students. The results are unchanged. Accordingly, we conclude that the results are not driven by the (uneven) distribution of participants' class standing among experimental conditions.

TABLE 1
Participant Demographics by Experimental Setting

Experimental Setting[a]	Gender	Age	Academic Class	Average Years of Audit Experience (range)	Average Years of Work Experience (range)
EIB/CF (n = 32)	Male = 15 Female = 17	< 20 = 0 20–24 = 28 25–29 = 2 30–34 = 1 > 34 = 1	Junior = 1 Senior = 25 5th year = 6	.331 (0–5)	4.422 (0–18.5)
EIB/NCF (n = 28)	Male = 13 Female = 15	< 20 = 0 20–24 = 19 25–29 = 4 30–34 = 4 > 34 = 1	Junior = 0 Senior = 17 5th year = 11	.563 (0–7)	5.744 (0–26)
ENB/CF (n = 24)	Male = 8 Female = 16	< 20 = 1 20–24 = 15 25–29 = 4 30–34 =1 > 34 = 3	Junior = 0 Senior = 18 5th year = 6	.167 (0–2.5)	6.510 (.25–25.5)
ENB/NCF (n = 24)	Male = 8 Female = 16	< 20 = 0 20–24 = 15 25–29 = 5 30–34 = 2 > 34 = 2	Junior = 0 Senior = 18 5th year = 6	.101 (0–1)	6.91 (0–25)
FTFB/CF (n = 40)	Male = 19 Female = 21	< 20 = 0 20–24 = 30 25–29 = 7 30–34 =1 > 34 = 2	Junior = 1 Senior = 35 5th year = 4	.071 (0–2)	6.46 (0–29)
FTFB/NCF (n = 40)	Male = 16 Female = 24	< 20 = 1 20–24 = 21 25–29 = 7 30–34 = 5 > 34 = 6	Junior = 2 Senior = 36 5th year = 2	.356 (0–8)	7.10 (0–30.5)

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[a] EIB = electronic interactive brainstorming;
ENB = electronic nominal brainstorming;
FTFB = face-to-face brainstorming;
CF = content facilitation; and
NCF = no content facilitation.

the Helecom Communications case (i.e., they were given the opportunity to refresh their understanding of the audit client and its industry), and were then asked to make an initial fraud risk assessment based on their reading of the case. All participants were told that they were part of a four-person audit team along with their assigned rank (i.e., staff, senior, manager, or partner). Participants then brainstormed for 20 minutes about the fraud risk factors present in the Helecom Communications case (Stage 2). They were told to focus exclusively on fraudulent financial reporting and to generate ideas about how and where the Helecom Communications financial statements might be susceptible to material misstatement due to fraud and how fraud might be concealed. Participants in the electronic interactive brainstorming treatment could input ideas simultaneously and see the ideas of other team members in real time. Participants in the electronic nominal brainstorming treatment input their ideas individually and were told that the ideas of all audit team members would be combined at the end of the brainstorming session. Participants in the face-to-face treatments met as a team in individual breakout rooms and verbally discussed ideas, with one assigned member acting as a scribe to type up ideas using a laptop computer.

In Stage 3, participants in the no-facilitation treatments continued to brainstorm as they had in Stage 2 for an additional 20 minutes, while participants in the facilitation treatments who brainstormed electronically were provided with a total of five system-fed prompts, at the rate of one prompt every four minutes. For participants brainstorming face-to-face, the prompts were printed on large sheets of paper, which the session administrator taped to a wall in the breakout room. The timing of the prompts in the face-to-face treatment condition was identical to the timing of the system-fed prompts. During the facilitated brainstorming period for face-to-face teams, after four minutes had elapsed, the existing fraud prompt sheet was removed and the next prompt sheet was taped on the wall in each breakout room.

The five prompts address issues emphasized in SAS No. 99. The ordering and content of these prompts was not specifically targeted to Helecom Communications. Three of the prompts focus on the factors associated with the "fraud triangle" concept. The remaining two prompts reminded participants to consider the risks of improper revenue recognition and management override of internal control. SAS No. 99 states that "the auditor should ordinarily presume that there is a risk of material misstatement due to fraud relating to revenue recognition" (AICPA 2002, 20). SAS No. 99 also states that "Even if specific risks of material misstatement due to fraud are not identified by the auditor, there is a possibility that management override of controls could occur, and accordingly, the auditor should address that risk" (AICPA 2002, 20). The complete text of the five content facilitation prompts is shown in Table 2.

In Stage 4, all participants were given ten minutes to review the log of fraud risk ideas generated by their audit team. For electronic brainstorming teams, the log of ideas represented what each team member had typed over the entire 40 minutes brainstorming session (Stages 2 and 3 combined). For teams brainstorming face-to-face, the ideas typed up by the team scribe on the laptop were printed and a copy was given to each team member to review. Participants were then asked to make a second fraud risk assessment for Helecom Communications. Finally in Stage 5, participants provided demographic information and answered several manipulation check questions.[11]

Dependent Measures and Data Coding

The dependent measures are (1) fraud brainstorming effectiveness, defined as the number of relevant fraud factors identified, and (2) the change in fraud risk assessment, defined as the difference between the pre-brainstorming and post-brainstorming fraud risk assessment on a scale of 1 to 100 percent indicating the probability of a material misstatement in financial reporting for Helecom Communications.

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[11] Since the manipulations in the electronic brainstorming conditions were computer-delivered and were therefore somewhat subtle, the post-experiment questionnaire in these conditions included four questions designed to ascertain the salience of the manipulations. The questions assessed whether participants recognized the nominal and interactive conditions and also the on-screen fraud prompts. In all audit teams except one, at least three of the four questions were correctly answered by all team members. The results are qualitatively similar with and without the one team in which participants failed the manipulation check questions. Administrators were either directly or indirectly present in every experimental session to ensure that participants attended to the experimental tasks.

TABLE 2
Content-Facilitation Prompts Provided to Participants in the Facilitation Condition

Order of Display	SAS No. 99 Reference	Prompts Adapted from SAS No. 99
1	"Incentives/Pressures" Concept of the Fraud Triangle	As you continue to generate fraud risk factors, consider whether management or other employees have an incentive or are under pressure to commit fraudulent financial reporting (e.g., excessive pressure exists for management to meet the requirements or expectations of third parties).
2	"Opportunities" Concept of the Fraud Triangle	As you continue to generate fraud risk factors, consider whether there are opportunities to commit fraudulent financial reporting (e.g., the nature of the industry or the entity's operations provides opportunities to engage in fraudulent financial reporting; internal control components are deficient).
3	"Attitudes/Rationalizations" Concept of the Fraud Triangle	As you continue to generate fraud risk factors, consider whether there are risk factors present reflective of attitudes/rationalizations by board members, management, or employees that allow them to engage in and/or justify fraudulent financial reporting.
4	Revenue Recognition	As you continue to generate fraud risk factors, consider the risk of improper revenue recognition.
5	Management Override	As you continue to generate fraud risk factors, consider the risk of management override of internal control.

Two accounting Ph.D. candidates in the final stages of the doctoral program served as coders. One of the coders had several years of audit experience at a Big 4 public accounting firm. Coders were provided a database containing all items generated by the participants during the fraud brainstorming session. The coders, who were blind to the research hypotheses, were instructed to identify duplicate items within audit teams, independently coding each non-duplicate item as either a relevant fraud risk factor or an irrelevant fraud risk factor. The coders were provided with a solution to the case containing 53 fraud risk factors. The solution was developed by the researchers using teaching notes in Ballou and Mueller (2005b) as well as SAS No. 99. Coders were instructed to classify all responses appearing on the solution as relevant. Coders were also instructed to use professional judgment in situations in which a response was not on the official solution but appeared reasonable and appropriate to the specific facts in the Helecom case.[12]

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[12] The authors of the Helecom Communications case recognize in the teaching notes that "the requirements posed in the case do not have a finite list of correct responses" (Ballou and Mueller 2005b, 39). They also state: "Determining whether a response is appropriate and complete should be measured in terms of whether the student has considered relevant known facts and demonstrated an understanding of how those facts may influence audit decisions" (Ballou and Mueller 2005b, 39).

Coders independently classified the individual items and then reconciled any discrepancies in their classification. Prior to reconciling discrepancies, the Cohen's Kappa coefficient—a measure of inter-rater reliability—was 0.55 for the 2,469 items coded. Landis and Koch (1977) deem a Kappa coefficient in the range of 0.41 to 0.60 to be "moderate" agreement, while a Kappa coefficient of 0.61 to 0.80 is considered to be "substantial" agreement.

IV. RESULTS

Hypotheses Tests

Hypothesis 1 predicts that brainstorming effectiveness, specifically total relevant fraud risks identified, will be greater for teams brainstorming electronically than for teams brainstorming face-to-face. The mean total number of relevant fraud risks for electronic and face-to-face brainstorming teams is shown in Table 3. As shown in Table 4, Panel A, the ANOVA reveals significant differences between treatments ($F = 19.78$, $p < 0.001$). To test H1, we perform planned contrasts to compare each of the two electronic brainstorming techniques (interactive and nominal) to face-to-face brainstorming. The results shown in

TABLE 3
Mean Total Relevant Fraud Risks
Electronic versus Face-to-Face Brainstorming Groups
(Standard Deviation in Parentheses)

Nominal Brainstorming Groups	Interactive Brainstorming Groups	Face-to-Face Brainstorming Groups
30.31	28.71	15.45
(8.65)	(9.8)	(4.50)
n = 13	n = 14	n = 20

TABLE 4
Effects of Brainstorming Mode (Test of H1 and H2)

Panel A: ANOVA Results for Mean Total Relevant Fraud Risks

Source of Variance	df	Mean Square	F-statistic	p-value
Main Effects				
Brainstorming Mode[a]	2	1139.584	19.783	< 0.001
Explained (model)	2	1139.584	19.783	< 0.001
Residual (error)	44	57.604		
Adjusted $R^2 = .450$				

Panel B: Results of Planned Contrasts between Brainstorming Modes

Contrasts	Difference in Means	p-value
Electronic Interactive (28.71) − Face-to-Face (15.45)	13.26	< .001
Electronic Nominal (30.31) − Face-to-Face (15.45)	14.86	< .001
Electronic Interactive (28.71) − Electronic Nominal (30.31)	−1.60	1.000

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[a] Participants were randomly assigned to either a face-to-face, electronic interactive, or nominal brainstorming mode.

Panel B of Table 4 indicate that both electronic interactive and nominal brainstorming are significantly more effective than face-to-face brainstorming, in terms of the number of relevant fraud risks identified. Thus, H1 regarding the effectiveness of electronic brainstorming over face-to-face brainstorming is supported.

Hypothesis 2 tests the null hypothesis of no difference in brainstorming effectiveness, specifically the number of relevant fraud risks identified, between the electronic nominal and electronic interactive brainstorming techniques. The mean total number of relevant fraud risks for the electronic brainstorming teams, by brainstorming technique is shown in Table 3. Those who participated in electronic nominal brainstorming have an average of 30.31 relevant fraud risks, while those who participated in electronic interactive brainstorming have an average of 28.71 relevant fraud risks. As shown in Table 4, Panel B, the planned contrast results indicate that the difference in the number of relevant fraud risks identified between electronic nominal and electronic interactive teams is statistically insignificant. Thus, the results do not reject null H2. Additional analyses reveal that the insignificant results for H2 are attributable to a very small effect size (partial $\eta^2 = 0.008$) and low observed power (0.071), given the relatively small cell sizes ($n_{\text{nominal}} = 13$; $n_{\text{interactive}} = 14$).

Hypothesis 3 predicts that brainstorming effectiveness will be higher for those who receive content facilitation relative to those who do not receive content facilitation. The mean number of relevant fraud risks for the pre- and post-facilitation periods, between those receiving facilitation and those not receiving facilitation, and between face-to-face, nominal, and interactive teams is shown in Table 5. Also shown in Table 5 are the estimated marginal means for each condition, after adjusting for the effect of the covariate—the relevant fraud risks generated in the first 20 minutes of brainstorming.

An ANCOVA was conducted for total number of relevant fraud risks, with brainstorming performance in the first 20 minutes as a covariate to control for fraud risk ideas generated prior to receiving the facilitation treatment.[13] As shown in Table 6, ANCOVA results on total relevant fraud risks reveal an insignificant interaction ($F = .467$, $p = .630$) and a significant main effect for both brainstorming mode (face-to-face, nominal, or interactive) and facilitation. Consistent with H3, the main effect for facilitation treatment is significant ($F = 20.56$, $p < 0.001$). Participants who received content facilitation generated a significantly higher quantity of relevant fraud risk factors than those who did not receive content facilitation under face-to-face brainstorming ($\bar{x}_{\text{facilitated}} = 22.50$; $\bar{x}_{\text{not-facilitated}} = 15.57$), electronic nominal brainstorming condition ($\bar{x}_{\text{facilitated}} = 29.06$; $\bar{x}_{\text{not-facilitated}} = 24.71$) and the electronic interactive brainstorming condition ($\bar{x}_{\text{facilitated}} = 28.88$; $\bar{x}_{\text{not-facilitated}} = 23.59$). Thus, H3 regarding the effectiveness of content facilitation is supported.

Hypothesis 4 predicts that in a scenario in which significant fraud risks are present, auditors' fraud risk assessments will increase after participating in a fraud brainstorming session. To determine whether brainstorming increases participants' awareness of fraud risk factors, we compare the initial fraud risk assessment in Stage 1 (pre-brainstorming and pre-facilitation treatment) to the final fraud risk assessment in Stage 4 (post-brainstorming and post-facilitation treatment). The initial fraud risk assessment has a mean value of 56.79 percent while the final fraud risk assessment has a mean value of 76.49 percent. A paired-

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[13] Participants receiving content facilitation brainstormed for the first 20 minutes without facilitation and for the second 20 minutes with facilitation. Participants not receiving content facilitation brainstormed for the entire 40 minutes without any facilitation. Using the fraud risks generated in the first 20 minutes as a covariate controls for variations in pre-facilitation brainstorming effectiveness between groups receiving facilitation and those not receiving facilitation.

TABLE 5
Mean Number of Relevant Fraud Risks By Treatment Condition and Facilitation Period
(Standard Error of the Mean in Parentheses)

		Number of Relevant Fraud Risks Identified			
Facilitation Mode	**Brainstorming Mode**	**No Facilitation Period (First 20 Minutes)**	**Facilitation Period (Second 20 Minutes)**	**Marginal Means: Actual**	**Marginal Means: Adjusted**[a]
Teams receiving content facilitation	Face-to-Face	11.70 (.496) n = 10	7.50 (.687) n = 10	19.10 (.849) n = 10	22.50 (1.20) n = 10
	Nominal	14.29 (1.58) n = 7	14.57 (1.48) n = 7	28.86 (2.11) n = 7	29.06 (1.39) n = 7
	Interactive	12.63 (1.59) n = 8	14.00 (1.60) n = 8	26.63 (2.27) n = 8	28.88 (1.32) n = 8
	Facilitation Total	12.72 (3.518) n = 25	11.56 (4.805) n = 25	24.24 (1.29) n = 25	26.81 (.765) n = 25
Teams not receiving content facilitation	Face-to-Face	11.40 (.600) n = 10	3.50 (.477) n = 10	11.80 (.772) n = 10	15.57 (1.21) n = 10
	Nominal	20.33 (3.52) n = 6	11.67 (1.78) n = 6	32.00 (4.76) n = 6	24.71 (1.64) n = 6
	Interactive	20.83 (3.55) n = 6	10.67 (2.52) n = 6	31.50 (2.21) n = 6	23.59 (1.67) n = 6
	No Facilitation Total	16.41 (7.695) n = 22	7.68 (5.481) n = 22	22.68 (2.86) n = 22	21.29 (.879) n = 22
	Overall Total	14.45 (.887) n = 47	9.74 (.793) n = 47	23.51 (1.49) n = 47	24.05 (.555) n = 47

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[a] Estimated marginal means after adjusting for the effect of the covariate, relevant fraud risks generated in first 20 minutes, evaluated in the model at a value of 14.45 (mean total relevant fraud risks in first 20 minutes).

samples t-test reveals that this increase in the pre- to post-brainstorming fraud risk assessment is statistically significant ($t = 15.02$, $df = 187$, $p < 0.001$). These results support H4. While it is not surprising that post-brainstorming fraud risk assessments are higher in an audit environment where a significant number of fraud risks are present, these results validate the intent of SAS No. 99. Specifically, from an audit effectiveness perspective, the results for H4 indicate that the SAS No. 99 fraud brainstorming requirement helps mitigate the risk of understating fraud risk when significant fraud risks are present.

TABLE 6
Effects of Content Facilitation (Test of H3)

ANCOVA Results for Mean Total Relevant Fraud Risks

Source of Variance	df	Mean Square	F-statistic	p-value
Main Effects				
Brainstorming Mode[a]	2	244.192	18.042	< 0.001
Facilitation[b]	1	278.207	20.555	< 0.001
Covariate				
Relevant Pre-Facilitation Fraud Risks[c]	1	1613.340	119.199	< 0.001
Interaction				
Brainstorming Mode × Facilitation	2	6.316	.467	.630
Explained (model)	6	712.059	52.609	< 0.001
Residual (error)	40	13.535		
Adjusted $R^2 = .871$				

[a] Participants were randomly assigned to either a face-to-face, electronic interactive, or nominal brainstorming mode.
[b] Participants were randomly assigned to either receive content facilitation or no facilitation.
[c] Relevant ideas submitted by participants prior to facilitation treatment, i.e., during the first 20 minutes of brainstorming.

Supplemental Analyses

Results indicate that electronic brainstorming is significantly more effective than face-to-face brainstorming and that there is no significant difference in fraud brainstorming effectiveness between nominal and interactive electronic brainstorming teams. While brainstorming *effectiveness* is of primary interest, of secondary interest is the degree of brainstorming *efficiency*. Specifically, if electronic brainstorming teams also generate a significantly greater number of *irrelevant* fraud risk ideas relative to face-to-face teams, then it could be argued that the greater number of irrelevant fraud risks might lead to a dilution effect, as the relevant fraud risk ideas are factored into subsequent audit planning. To address this issue, we divide the number of relevant fraud risk factors for each team by the total number of fraud risk factors generated (relevant + irrelevant) as a measure of brainstorming efficiency. Electronic brainstorming teams have an average of 29.5 relevant ideas and 2.9 irrelevant ideas, whereas face-to-face teams have an average of 15.5 relevant ideas and 1.8 irrelevant ideas. Thus, for electronic brainstorming teams, brainstorming efficiency is 91 percent (29.5/32.4) as compared to 90 percent (15.5/17.3) for face-to-face teams. ANOVA results (not tabulated) reveal that this difference is not statistically significant ($F = 1.44$, $p = 0.237$). Comparing electronic interactive to electronic nominal brainstorming teams, this efficiency measure is 94 percent for interactive teams and 90 percent for nominal teams. ANOVA results (not tabulated) reveal that this difference is not statistically significant ($F = 2.563$, $p = 0.122$). These results indicate that the brainstorming effectiveness gains attributable to electronic brainstorming and content facilitation are not at the cost of brainstorming efficiency, defined in terms of the ratio of relevant-to-total fraud risks identified.

V. DISCUSSION AND CONCLUSION

The purpose of this study is to investigate whether computer-mediated brainstorming and content facilitation can improve the effectiveness of a SAS No. 99 fraud brainstorming session. We compare electronic fraud brainstorming to traditional face-to-face brainstorming, finding that electronic brainstorming is significantly more effective than face-to-face brainstorming. Electronic brainstorming resulted in audit teams identifying significantly more relevant fraud risks compared to teams brainstorming face-to-face. Supplemental analyses reveal that the efficiency of electronic brainstorming is approximately the same as the efficiency of face-to-face brainstorming, with efficiency being defined as the ratio of relevant-to-total fraud risks identified during brainstorming. In addition, in all treatment conditions there is a statistically significant increase in participants' fraud risk assessments after brainstorming.

The findings of this study suggest that both the interactive and nominal forms of electronic brainstorming can be used to increase the fraud detection capabilities of auditors under GAAS and PCAOB Auditing Standards as compared to traditional face-to-face brainstorming. The use of electronic brainstorming would also allow public accounting firms to conduct brainstorming sessions with participants in remote locations, generating an electronic record of the fraud risk factors identified. Although we find that electronic communication is superior to face-to-face communication in a team setting in which all participants have the same objective, it is important to note this finding may not hold in other contexts. For example, Brazel et al. (2004) find that in an audit review setting involving an interaction between two auditors, face-to-face communication resulted in more effective audit outcomes. In a setting involving transfer price negotiations, Kachelmeier and Towry (2002) find that negotiated transfer prices reflect fairness-based price concessions when negotiations are conducted face-to-face with unrestricted communication but not when they are conducted over a computer network.

We also examine the relative effectiveness of electronic interactive brainstorming compared to electronic nominal brainstorming, finding no statistically significant difference. Apparently, at least in the context of the participants and task in the current study, neither technique is dominant. One interesting implication of the insignificant difference between electronic nominal and interactive brainstorming is that fraud brainstorming session(s) can be conducted in an asynchronous mode, allowing auditors to participate in the brainstorming "session(s)" at any time from any place.[14] The difficulty of coordinating the schedules of all audit team members (especially managers and partners) for the purpose of fraud brainstorming session(s) can thus be avoided without a loss in brainstorming effectiveness.

A significant innovation in our study is the automation of content facilitation, by which the electronic brainstorming system feeds relevant prompts for auditors involved in the brainstorming session to consider. We find that the use of automated content facilitation results in a significantly greater number of relevant fraud risk factors being identified by audit teams. This finding is consistent with existing IS research findings on the effects of person-led facilitation in settings other than auditing (e.g., Anson et al. 1995; Niederman et al. 1996; Dennis and Wixom 2001). We also find that content facilitation is effective for teams brainstorming face-to-face, suggesting that the relatively simple method of providing fraud prompts on large posters during a face-to-face brainstorming session can improve brainstorming effectiveness. Our results regarding the effectiveness of content facilitation should encourage public accounting firms to use the technique and to devote resources to the development of the most effective fraud-related prompts, incorporating the talents of their most experienced, knowledgeable, and creative auditors.[15]

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[14] SAS No. 99 requirements apply to audit teams of all sizes, and SAS No. 99 does not mandate that participants in the brainstorming session be present at the same time and location during the brainstorming process.

Our findings also validate the premise underlying the SAS No. 99 brainstorming requirement, in that simply participating in a fraud brainstorming session heightens the auditor's sensitivity to fraud when significant fraud risks are present. This elevated fraud risk assessment should translate into a more effective audit as auditors comply with the guidance in SAS No. 99 and adjust the nature, timing, and extent of audit procedures to respond to the fraud risks identified.

As with all experiments, this study is subject to limitations that restrict the generalizability of the results. Participants were not trained on the GSS system used in this study. Thus, participants may have taken time to understand the system as they generated fraud risk factors. With training and experience using the GSS, the productivity of computer-mediated teams could be higher than what was observed in our study. The use of students as surrogates for practicing auditors is another potential limitation, although the study incorporated several measures designed to mitigate potential problems stemming from the use of students. First, we used a realistic case that was designed specifically for accounting students, containing detailed information about the audit client and its industry. Second, we determined that the initial knowledge differential about the case material based on class ranking was similar to what would be expected within an actual audit team. Third, the participants were near completion of a rigorous accounting program, they were educated about auditing (including fraud brainstorming), and their demographic characteristics were typical of what is commonly found among entry-level auditors in practice. Further, as indicated in the "Method" section, Peecher and Solomon (2001) suggest that the failure to incorporate audit experience as a factor in an audit decision-making experiment causes a threat to (internal) validity only if experience is theoretically expected to *interact* with a factor of interest in the study. Prior research in psychology and auditing indicates that experience is unlikely to interact with either communication mode or facilitation—the two main factors of interest in our study (Libby and Frederick 1990; Moeckel 1990; Paulus et al. 1995).

Our findings indicate that electronic interactive and nominal brainstorming are similarly effective, but further research is necessary to confirm (or disconfirm) our failure to reject the null hypothesis of no difference between the two techniques. Additionally, we used a case that had numerous fraud risk factors associated with the organization. Future research could extend the current study by adding a case in which no, or limited, fraud risk factors are present to determine if participants adjust their fraud risk assessments appropriately. Finally, our study examined the impact of introducing content facilitation *after* participants had engaged in a period of brainstorming without any facilitation. Future research could investigate the effect of introducing content facilitation at the beginning of the session.

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[15] The researchers exposed four audit practitioners (i.e., a partner, a manager, a senior, and a staff auditor) from an international public accounting firm to the GSS to obtain input as to the feasibility of utilizing the technology in their audit practice and their willingness to embrace the technology. The practitioners engaged in a fraud brainstorming session structured identically to the electronic interactive-facilitated treatment in this study. A debriefing session following the brainstorming session revealed that the auditors found the GSS effective, practical, and efficient. The partner called the system "fantastic" and inquired about the possibility of using the system on an upcoming audit following PCAOB Auditing Standards. The partner also commented favorably about the documentation aspect of the system since all fraud risk ideas generated during the session were automatically captured. The staff auditor stated that he felt less inhibited using the GSS. He stated that he commonly did not talk much because he did not want to waste people's time in a brainstorming session. Using this technology he felt free to just "throw ideas out there." The partner subsequently stated that he wanted his lower-ranked auditors to actively participate in the brainstorming session and the GSS system seemed to facilitate that.

To conclude, our results support the use of computer-assisted fraud brainstorming and the use of prompts to spur the identification of relevant fraud risk factors. Our findings should be of interest to practicing auditors looking for the most effective and efficient ways to conduct fraud brainstorming sessions.

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Balancing the Dual Responsibilities of Business Unit Controllers: Field and Survey Evidence

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ABSTRACT: We examine how business unit (BU) controllers balance their dual roles of providing information for both local decision-making (local responsibility) and corporate control (functional responsibility). The existing literature suggests that organizations can improve the quality of financial reporting and internal controls by increasing the emphasis on the functional responsibility of BU controllers. In this study, we rely on prior literature and insights from field interviews to argue that such an increase also leads to role ambiguity and conflict, which in turn can lead to an increased tolerance of data misreporting. We test our predictions using survey responses of 134 BU controllers. As predicted, we find that the emphasis on the functional responsibility of BU controllers is positively associated with role conflict and role ambiguity, both of which are related to data misreporting at the BU level.

Keywords: *controllers; role conflict; role ambiguity; earnings management.*

Data Availability: *Data used in this study are not publicly available due to confidentiality agreements with the managers and controllers participating in this study.*

I. INTRODUCTION

The governance failures at Enron, WorldCom, Tyco, and other companies have cast doubt on the effectiveness of institutions that should protect company assets from managerial opportunism. Several studies examine the role of auditors in such

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governance failures and discuss how auditor independence can be compromised when auditors provide both auditing and consulting services (Francis 2006; Kinney et al. 2004; Ashbaugh et al. 2003; Frankel et al. 2002). The corporate governance literature shows that directors also experience a conflict between their "watchdog" and "advisory" roles (Adams and Ferreira 2007; Bushman and Smith 2001). Our study complements this literature by examining the role of business unit (BU) controllers in the financial reporting process.

As is the case for auditors and directors, BU controllers balance conflicting responsibilities and have a great influence on the integrity of financial reporting. BU controllers' *functional responsibility* is to "fairly and objectively" report on the economic situation of their unit and to facilitate corporate control. BU controllers are also members of local management teams, and their *local responsibility* is to provide BU management with information to facilitate strategic and operational decision-making (Siegel and Sorensen 1999; Jablonsky and Keating 1998). Prior literature suggests that BU controllers better facilitate corporate control and prevent data misreporting when firms increase the emphasis on their functional responsibility (e.g., Indjejikian and Matějka 2006; San Miguel and Govindarajan 1984). In contrast, our study shows that increasing the emphasis on the functional responsibility of BU controllers can paradoxically have detrimental effects on controllers' ability to act as a "corporate watchdog."

To motivate our predictions, we rely on extensive, qualitative data collected through interviews with controllers and other financial managers. We find that controllers perceive their local responsibilities as more important than their functional duties. As a result, strengthening their functional ties makes it harder for them to reconcile their dual roles, exposing them to higher levels of role conflict and role ambiguity. Relying on organizational psychology literature on role stress and dysfunctional behavior, we further argue that greater controller role conflict and role ambiguity can lead to data misreporting at the local level (e.g., biasing of forecasts, earnings smoothing). Consequently, a greater emphasis on the functional responsibility of BU controllers can indirectly lead to an increase instead of a decrease in data misreporting practices.

We test our hypotheses using survey data from 134 BU controllers of large- and medium-sized companies in The Netherlands. The results support our predictions and caution against overly emphasizing the functional responsibility of BU controllers. First, we find that controllers working under stricter functional ties report higher levels of role conflict and role ambiguity, which are, in turn, positively associated with data misreporting at the local level. In addition, we find that increasing the emphasis on the functional responsibility of BU controllers compromises their role as service providers to local managers. Thus, our evidence casts doubt on the widely held belief that the emphasis on the functional responsibility of BU controllers alleviates data misreporting issues and also suggests that increasing this emphasis can reduce the quality of BU decision-making.

The results of our study imply that BU controllers, one of the key accounting institutions safeguarding the integrity of financial reporting, are not immune to role stress and are likely to resort to data misreporting as a dysfunctional way of dealing with it. Although prior organizational behavior studies find that role stress is associated with dysfunctional behavior such as absenteeism, poor productivity, or lying (e.g., Lau et al. 2003; Grover 1993; Jackson and Schuler 1985), our study is the first of which we are aware to show that role conflict and role ambiguity can adversely affect the quality of financial reporting.

The next section develops our hypotheses. Section III describes data collection and our empirical measures. Section IV presents the results of testing our theoretical model, and the last section discusses our findings, conclusions, and potential limitations.

II. HYPOTHESES

Our study examines the consequences of increasing the emphasis on the functional responsibility of BU controllers. In this section, we rely on prior literature and qualitative data from 48 field interviews to develop our hypotheses. We first describe BU controllers' responsibilities and subsequently discuss how emphasis on functional responsibilities of BU controllers affects their ability to support local decision-making and to prevent data misreporting.

Emphasis on the Functional Responsibility of BU Controllers

It is well known that BU controllers have two distinct sets of responsibilities (e.g., San Miguel and Govindarajan 1984; Simon et al. 1954). First, their *local responsibility* is to directly add value to their BU by helping managers identify profitable action strategies (also referred to as the "business partner" or "service-aid" role). Second, their *functional responsibility* is to contribute to the organization by providing higher-level managers with information about the BU and its activities (also referred to as the "corporate policeman" or "watchdog" role). A representative comment by a business group controller describes the functional responsibility of BU controllers as follows:

> When the [BU manager] wants to show other figures, paint a nicer picture of the company, and the controller does not agree with that, he has to first of all discuss it with his [BU manager] and when they do not come to a conclusion, he will always come to me and tell me that he does not agree with the figures he is forced to report and then we [business group executives] go to [the BU manager] ...

We also know from prior literature that the *emphasis on the functional responsibility* of BU controllers varies over time (Burns and Baldvinsdottir 2005; Ahrens and Chapman 2000). In the past, controllers were often viewed as "passive bean counters" who performed their functional responsibilities with little involvement in local decision-making. Over time they have come under pressure to improve their "business orientation" and to participate more in operational and strategic decision-making (Siegel and Sorensen 1999). In the post-Sarbanes-Oxley environment, however, their main duties may have "reverted to the old 'bean counter' role" (Sinnett 2007).

It is not quite clear from prior research what happens when firms increase the emphasis on the functional responsibility of BU controllers (e.g., by centralizing policies regarding hiring, promoting, or firing of BU controllers; by increasing the intensity of formal and informal communication between a BU controller and the functional superior). Few studies test explicitly how the emphasis on the functional responsibility of BU controllers affects their abilities to support local decision-making and to prevent misreporting of information BUs provide to higher-level managers.

Effect on Decision-Making Support

It has often been argued that functional responsibilities of BU controllers are difficult to reconcile with their role of supporting local decision-making (e.g., Granlund 1998b; McGregor et al. 1989). Prior studies discuss how local and corporate needs are often incongruent (Sathe 1982); how local and functional superiors have different ideas about the relative importance of controllers' tasks (Pierce and O'Dea 2003) and how time limits make it difficult to live up to all demands (Hopper 1980). This would suggest that the dual responsibilities of BU controllers are substitutes—performing the functional role makes it harder to perform the local role and *vice versa.*

However, an alternative view is that complementarities exist between controllers' dual responsibilities. Sathe (1982) argues that "strong" controllers are able to overcome the apparent conflict between their responsibilities and that by getting closely involved in local decision-making, they will also be in a better position to perform their functional duties. Supporting this point, one corporate controller in our study advocated involvement of BU controllers in local decision-making exactly because their functional responsibilities are very important and because otherwise they "would run the risk of not being taken seriously within the business group, of being by-passed with certain information, and of getting a mere registration function."

While both views above are plausible, Indjejikian and Matějka (2006) find that high emphasis on the functional responsibility of BU controllers is negatively associated with BU managers' satisfaction with services provided by their BU controllers. This evidence suggests that complementarities between controllers' responsibilities are not large enough to dominate inherent tensions between the dual roles discussed earlier. This leads to our first hypothesis:

H1: The emphasis on the functional responsibility of BU controllers is negatively associated with decision-making support of local management.

Effect on Data Misreporting

The key functional responsibility of BU controllers is to prevent misreporting of information presented to higher-level executives. Yet, the extent to which BU controllers are able and willing to objectively report about BU affairs varies. For example, prior literature finds evidence of biasing of forecasts, building slack in performance targets, and the manipulation of accruals (Van der Stede 2000; Guidry et al. 1999; Collins et al. 1987). In our field study, one BU controller described instances of misreporting as follows (referring to a period prior to the acquisition of his BU by another corporation):[1]

> [T]he BU controller was very close to the BU manager, he stood together with the BU manager to defend the budget against the business group. The same was true for investments, both tried to sell them. ... For example, you could decide towards the end of the year to expense less and to capitalize more investments to meet your target.

The agency literature on multi-tasking can shed some light on controllers' role in preventing misreporting (Datar et al. 2001; Feltham and Xie 1994; Holmström and Milgrom 1991). BU controllers can be viewed as rational agents who allocate their effort over multiple tasks. Increasing the emphasis on the functional responsibility (e.g., by increasing the extent to which compensation and appointment of BU controllers is decided by functional superiors rather than by BU managers) strengthens implicit and explicit incentives of BU controllers to allocate effort to their functional task(s). This change in effort allocation alleviates data misreporting issues albeit at the cost of less effort being devoted to the local task(s) of supporting local managers.[2]

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[1] Other types of misreporting we encountered include biasing latest estimates, being "too optimistic" with proposed budgets, or "dumping a large provision that was not budgeted for in non-operating items." In the words of a BU controller: "Of course, there a number of rules that you follow, yet there is always some room, some gray areas and those often really matter."

[2] Similarly, prior literature on the provision of pre-decision information to managers (e.g., Baiman and Sivaramakrishnan 1991) motivates a characterization of BU controllers as a mechanism for regulating information asymmetry between the BU and corporate level. Increasing the emphasis on the functional responsibility increases vertical information flows and reduces BU managers' informational advantage. This diminishes BU managers' ability to extract rents from the organization.

Existing empirical evidence is consistent with the view based on agency theory. San Miguel and Govindarajan (1984) show that when firms emphasize the functional responsibility of BU controllers (by allocating more authority to the corporate controller), their internal audit function is relatively less concerned with compliance issues. Indjejikian and Matějka (2006) find that increasing the emphasis on the functional responsibility of BU controllers is associated with less slack in BU performance targets. This motivates our second hypotheses:

H2: The emphasis on the functional responsibility of BU controllers is negatively associated with data misreporting.

The above prediction rests on the assumption that BU controllers act as impersonal information providers who are indifferent about how they allocate their effort. However, psychological research suggests that individuals who need to balance dual responsibilities often experience tension in the form of role conflict or role ambiguity. A large stream of literature on role theory has identified both these forms of role stress as key determinants of organizational behavior (e.g., Tubre and Collins 2000; Ilgen and Hollenbeck 1991).[3]

Without any additional assumptions, role theory predicts a bell-shaped relation between the emphasis on the functional responsibility of BU controllers and role stress. At both extremes, i.e., if BU controllers performed only their local duties or only their functional duties, there would be little or no role conflict and role ambiguity, as there would be a clear set of expectations from a single interest group (BU or corporate management, respectively). On the other hand, role stress would be at its maximum at a point where the emphasis on functional responsibilities is about equal to the emphasis on local responsibilities of BU controllers.

We rely on our field observations to refine this general prediction of a non-monotonic relation between the emphasis on the functional responsibility and role stress. Based on the evidence presented below, we argue that BU controllers in our sample (i.e., controllers of relatively independent BUs of medium- and large-sized companies) perceive their local responsibility as the dominant one and have a strong sense of allegiance to local BU managers. In other words, BU controllers are not indifferent between their dual responsibilities—they recognize the importance of their functional duties, yet they much rather work on their local tasks and make themselves useful to BU managers with whom they interact on a day-to-day basis. For example, one BU controller said:

> The controller stands for the [BU] where he works. Of course, the controller also has a corporate responsibility within the corporation. So, in some sense, he always has two hats. If you ask me which hat I prefer, then I tell you I am an employee of the [BU].

Other BU controllers often make similar remarks when discussing their dual responsibilities: "Your primary loyalty is of course with the [BU] management team," or "I think that I am more useful for the [BU] than for the holding." Interestingly, functional superiors of BU controllers are aware that BU controllers and BU managers have a very close relationship and actively encourage that. Two representative quotes from business group controllers illustrate this point:

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[3] *Role conflict* is a form of distress that results either when a person in an organization experiences the simultaneous occurrence of two or more sets of role expectations, such that compliance with one would make compliance with the other more difficult, or when role pressures somehow violate the moral values or personal needs of the individual in question. *Role ambiguity* is a form of distress that arises if job demands are unclear, for example because several individuals communicate different expectations to a person regarding his or her work behavior (Kahn et al. 1964).

> Every BU has a BU controller ... He reports hierarchically to his BU manager and functionally to me. His office should be next to or opposite to the BU manager's office, they should almost share the same office.
>
> You have to allow some range for the game. You must not want to know everything. What you do have to know is whether they themselves know. For me, a [BU controller] and a [BU manager] are one party ... I treat them as one ... I do not want to sit in between them because they have to work together.

Thus, BU controllers' personal ambitions as well as role expectations of their local and functional superiors make BU controllers' local responsibilities paramount regardless of the emphasis on their functional responsibilities. Thus, if some firms increase the emphasis on the functional responsibilities, the "range for the game" becomes smaller and different forms of role stress are likely to arise because the "primary loyalty" of BU controllers is with the local management team.[4] We predict that:

H3a: The emphasis on the functional responsibility of BU controllers is positively associated with role conflict.

H3b: The emphasis on the functional responsibility of BU controllers is positively associated with role ambiguity.

Further, role conflict and role ambiguity are common antecedents of dysfunctional workplace attitudes and behaviors (Lau et al. 2003; Jackson and Schuler 1985; Fisher and Gitelson 1983). The reason is that individuals who perceive their work demands as conflicting and/or ambiguous react to this frustration by lowering their personal standards and ambitions, so that they become less reluctant to behave in ways that are socially undesirable or counterproductive. For example, Yetmar and Eastman (2000) find that role stress made accountants working in tax practice less sensitive to ethical issues and Grover (1993) and Grover and Hui (1994) find evidence that role stress increases the extent to which experimental subjects lie about their behavior.

In our setting, dysfunctional behavior could manifest itself when BU controllers compromise their functional duties and yield to pressure of local management to "paint a nicer picture" and misreport data presented to higher-level management (e.g., Davis et al. 2006). However, it is ultimately an empirical question to what extent high levels of stress can induce BU controllers to engage in data misreporting given that it constitutes a salient violation of the moral values and professional guidelines associated with the (management) accounting profession.[5] We therefore test the following hypotheses:

H4a: Role conflict is positively associated with data misreporting.

H4b: Role ambiguity is positively associated with data misreporting.

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[4] The following quote illustrates role stress experienced by a BU controller: "I used to have huge fights with my BU manager. I said in the first quarter—this is not going well, I am sure we are going to miss the target by millions unless something changes. Many BU managers have the tendency then to say: 'You know what you have to do, you have to shut up.' "

[5] For example, Harrell et al. (1989) find evidence that members of a professional association of internal auditors resisted biasing evaluations of internal controls systems as desired by management.

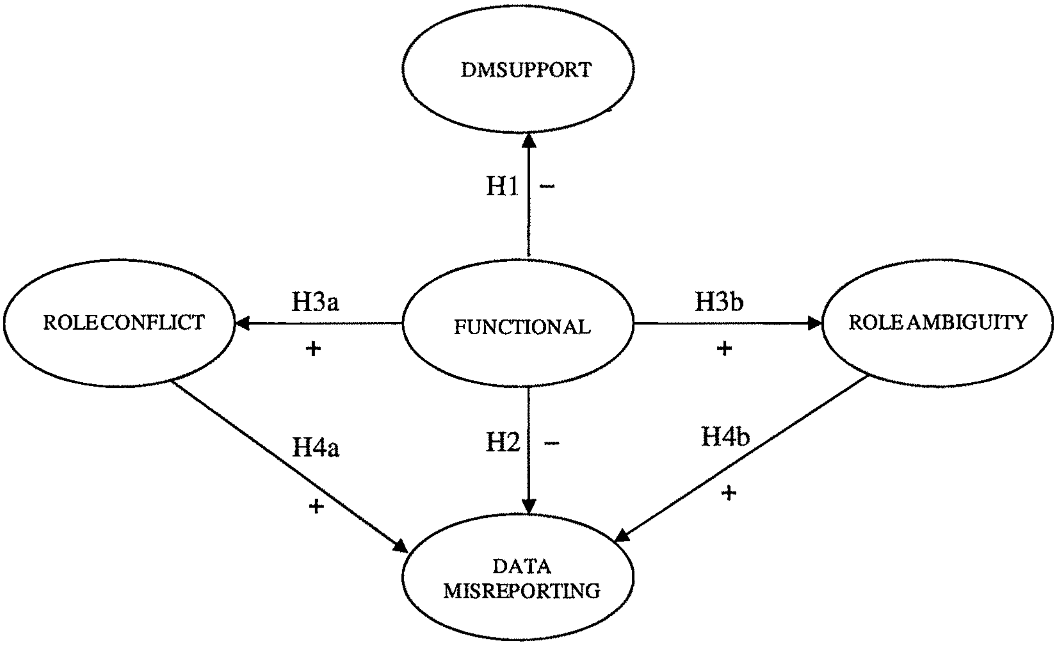
In summary, we expect that increasing the emphasis on the functional responsibility adversely affects BU controllers' ability to support local decision-making (H1). The implications for BU controllers' ability to prevent misreporting are less clear. Hypothesis 2 predicts that greater emphasis on the functional responsibility of BU alleviates misreporting. In contrast, H3 and H4 combined suggest that such emphasis can indirectly lead to an increase in data misreporting as a dysfunctional way for BU controllers to deal with role conflict and role ambiguity (see Figure 1).

III. RESEARCH DESIGN

Sample Selection and Survey Procedures

We surveyed controllers of decentralized units of large- and medium-sized companies in The Netherlands. In selecting our sample, we relied on contact information provided by the Controllers Instituut (CI), the professional organization of controllers in The Netherlands. Individuals holding the Dutch equivalent of the CMA title and Dutch CPAs working

FIGURE 1
Theoretical Model



H1: The emphasis on the functional responsibility of BU controllers (*FUNCTIONAL*) is negatively associated with decision-making support of local management (*DMSUPPORT*).
H2: The emphasis on the functional responsibility of BU controllers is negatively associated with data misreporting.
H3a: The emphasis on the functional responsibility of BU controllers is positively associated with role conflict.
H3b: The emphasis on the functional responsibility of BU controllers is positively associated with role ambiguity
H4a: Role conflict is positively associated with data misreporting.
H4b: Role ambiguity is positively associated with data misreporting.

in industry automatically become a member of the CI.[6] At the moment of the study, the CI member database contained 5,694 names.

As a first step in selecting the sample from the database, we defined the target population (Van der Stede et al. 2005) as controllers working for one specific unit of a medium or large for-profit company in The Netherlands. We restricted the target population to controllers working in The Netherlands to control for institutional and cultural differences (Granlund and Lukka 1998a; Chow et al. 1996). The restriction on company size was made to increase the likelihood that respondents were working in diversified companies with relatively autonomous units with their own controller. Specifically, we excluded members with a foreign address, members working for companies with less than 250 employees worldwide or not-for-profit organizations and members whose job title indicated they were not working as controllers of a specific BU (e.g., CMAs working as consultants, general managers, or corporate controllers). The final survey population consists of 1,063 CI members from which we randomly drew a sample of 314 members to participate in our study.

When designing and administrating our questionnaire, we closely followed the recommendations of Dillman (2000). The questionnaire was pre-tested with selected members of the target population, none of which participated in the final study. To gain the interest of the target population and to increase the response rate, an article was published in the professional journal that all members of the CI receive automatically on a two-month basis. This article announced the research project and described its relevance for practice without specifying the exact purpose of the study. A package containing the questionnaire, a hand-signed cover letter, a pre-stamped envelope, and a high-quality pen with the logo of the organization was sent out approximately one month after the publication of this article. Two follow-up mailings and a round of telephone calls succeeded the initial mailing.

This survey administration procedure resulted in the return of 186 (59.2 percent) questionnaires. We remove 49 respondents not working as controllers for a specific BU and four respondents because of missing data. This yields our final sample of 134 respondents (43 percent of those contacted) from 88 different companies. Tests show no significant differences between early and late respondents with regard to demographics and mean item scores, alleviating concerns about nonresponse bias. Also, we do not find significant company-level effects on any of our main variables and thus treat the 134 observations as independent.

Main Measures

The Emphasis on the Functional Responsibility of Controllers (FUNCTIONAL)

We adapt an instrument from prior literature (Indjejikian and Matějka 2006; San Miguel and Govindarajan 1984). Specifically, we measure the functional superior's influence on the BU controller's (1) performance evaluation, (2) selection and placement, (3) dismissal, and (4) work priorities. Table 1 presents the exact wording of the survey question and distribution of responses on all six items initially included. While there is substantial variation, mean scores on most items are close to the midpoint reflecting that BU managers and functional superiors "are about equally important." BU managers have greatest influence on performance evaluation, salary increase, and work priorities of BU controllers (more than 50 percent of sample respondents report that BU managers have greater influence in these areas than functional superiors). Functional superiors have relatively more influence on promotion and transfer of BU controllers (only about 30 percent of responses suggest that BU managers have greater influence than functional superiors).

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[6] The website http://www.ci.nl contains a more detailed description of the CI and its activities.

TABLE 1
Descriptive Statistics on Items Measuring the Emphasis on the Functional Responsibility of BU Controllers
(n = 134)

	Mean	1	2	3	4	5
Performance evaluation	2.54	43%	13%	10%	16%	19%
Salary increase	2.68	37%	16%	10%	16%	22%
Selection and placement	2.95	23%	13%	25%	22%	16%
Dismissal	2.84	24%	19%	25%	15%	18%
Promotion and transfer	3.11	14%	16%	30%	24%	16%
Work piorities	2.57	28%	24%	18%	22%	8%

Displayed is the frequency of responses to the following question:

> Some controllers report to both a local general manager and a functional superior (e.g., corporate controller, group controller). Please indicate the relative influence of these superiors in your situation with regard to the next five aspects of your job.

1 = functional superior has (almost) no influence;
2 = functional superior has less influence;
3 = both are about equally important;
4 = functional superior has more influence; or
5 = functional superior has (almost) complete influence.

Testing for unidimensionality of these items using confirmatory factor analysis (CFA), we find an acceptable fit (p = .30, RMSEA = .04, NNFI = 1.00) after dropping the second and fifth item in Table 1 ("Salary increase" and "Promotion and transfer") that correlate too closely with the first and fourth item, respectively. Reliability of the four-item construct is strong (α = .93). We calculate our measure of the emphasis on the functional responsibility of controllers (*FUNCTIONAL*) as an equally weighted average of scores on the four items.

Our field study observations further support validity of our measure in that they confirm that different companies, and even different business groups within the same company, can have different formal or informal policies regarding hiring, promotion, or firing of BU controllers. As an illustration, the following quotes contrast examples of decentralized and very centralized policies, respectively:

> The [BUs] take the initiative in appointing somebody and we [business group executives] test it once more, check it once more, and then approve. If I have some doubts and a [BU manager] says "I want to have that one," then I say, "I have doubts but I am going to stand behind you if that is the one you want to have."

> Management development policy for [accounting and finance] people is very centralized at [our company]. That means that controllers are appointed by a central committee [at the corporate level]. So, you stand in your own way if you as a controller misuse your position within the business group. That could eventually affect your career because you career is not in the hands of the business group manager but eventually in the hands of the central committee. The finance function plays quite an important role in the central committee. So, it is important to be friends with them.

Similarly, functional superiors of BU controllers can be more or less involved in BU affairs and their influence on what BU controllers do varies. One controller contrasted his experience in two different companies:

> [I also worked at another company] where they have a very different philosophy. Whether you are a controller or the [BU] CEO, there are people on the phone literally checking what you are doing. It is very explicit how you should deal with certain things. In [our company] ... there is no involvement from the corporate office. As long as you did things right it is OK. If things went wrong, then of course they would take more interest.

In conclusion, the emphasis on the functional responsibility of BU controllers is likely to vary across companies and BUs and along different dimensions. Both our field and survey evidence suggest that our measure described above (*FUNCTIONAL*) reflects a substantial part of that variation.

Decision-Making Support of Local Management (DMSUPPORT)

We use five items that refer to activities typically associated with the decision-making support role of BU controllers as suggested in the practitioner-oriented literature. Specifically, we ask about the importance of the following five tasks: (1) analyzing product/customer profitability, (2) evaluating investment opportunities, (3) developing local business strategy, (4) helping to meet local targets, (5) reducing costs and increasing earnings. Table 2 presents the question wording and distribution of responses. We find that BU controllers attribute on average high importance to their local tasks. Still, there are notable differences among the importance of different tasks. For example, 43 percent of BU controllers view their role in "developing cost saving and revenue increasing plans for the BU" as very important. In contrast, only 15 percent of them consider "finding new ways to meet BU targets" as very important.

We calculate a proxy for the importance of decision-making support of local management (*DMSUPPORT*) as an equally weighted average of scores on all five items. A CFA supports unidimensionality ($p = .51$, RMSEA = .00, NNFI = 1.00) and reliability is satisfactory ($\alpha = .74$).

TABLE 2
Descriptive Statistics on Items Measuring Local Decision-Making Support
($n = 134$)

	Mean	1	2	3	4	5
Analyzing product and customer profitability	3.86	3%	10%	19%	34%	34%
Development and evaluation of investment opportunities	3.66	4%	13%	21%	34%	27%
Developing new business unit strategy	3.88	4%	4%	20%	40%	31%
Finding new ways to meet business unit targets	3.52	2%	13%	30%	40%	15%
Developing cost saving and revenue increasing plans for the business unit	4.23	1%	4%	10%	42%	43%

Displayed is the frequency of responses to the following question:

Please indicate how important these activities are for you in your current job.

1 = very unimportant;
2 = relatively unimportant;
3 = averagely important;
4 = relatively important; or
5 = very important.

Data Misreporting (MISREPORT)

We rely on two questions with five items each to capture the extent to which data misreporting is likely to occur at the local level. The first question asks controllers how often they are under pressure from BU managers to make local performance look better by (1) transferring money between different accounts, (2) booking transactions late, (3) booking transactions early, (4) arranging for slack in performance targets, and (5) re-labeling line items. The second question asks controllers to what extent they find it acceptable to make local performance look better along the same dimensions. Relative to the first question, the second one relates more closely to our theoretical construct but it is also more likely to suffer from social desirability bias.[7]

Descriptive statistics on items measuring data misreporting are in Table 3. We note that the distribution of responses on the five items in Table 3 is similar across the two questions. Mean responses are quite low, suggesting that these practices are mostly regarded as relatively unacceptable. One exception is "arranging for slack in performance targets"—about two-thirds of our respondents consider it at least "sometimes acceptable." Further, we find that only two of the 134 respondents answer with the lowest possible score of 1 on all ten

TABLE 3
Descriptive Statistics on Items Measuring Data Misreporting
(n = 134)

	Mean	1	2	3	4	5
Question 1						
Transferring money between different accounts	1.76	52%	27%	16%	3%	2%
Booking transactions late	1.85	46%	31%	15%	6%	1%
Booking transactions early	1.71	51%	34%	10%	4%	1%
Arranging for slack in performance targets	2.85	10%	27%	38%	19%	6%
Re-labeling line items	1.68	60%	24%	8%	3%	4%
Question 2						
Tranferring money between different accounts	1.67	51%	34%	11%	3%	1%
Booking transactions late	1.60	55%	32%	10%	1%	1%
Bookin transactions early	1.64	54%	31%	13%	1%	1%
Arranging for slack in performance targets	2.89	3%	31%	41%	22%	3%
Re-labeling line items	1.79	47%	30%	21%	1%	1%

Displayed is the frequency of responses to the following two questions:

Question 1: Please indicate how often you are under pressure from business unit managers to do the five things mentioned below.

Question 2: Please indicate to what extent you find the five things mentioned below acceptable when it comes to you business unit's performance reports.

1 = never (acceptable);
2 = incidentally (acceptable);
3 = sometimes (acceptable);
4 = often (acceptable); or
5 = almost always (acceptable).

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[7] By also including the first question and indirectly asking about misreporting by BU controllers, we follow an approach commonly used in prior literature. For example, Merchant (1990) asks his respondents to indicate how often they, *or someone within their organization*, engaged in manipulative activities.

items. This suggests that the large majority of BU controllers in our sample have at least some experience with data misreporting.

We calculate our proxy for data misreporting (*MISREPORT*) as an equally weighted average of scores on all the ten items. Thus, we assume that high scores on any of these items suggest a greater likelihood of data misreporting. At the same time, we do not necessarily expect that data misreporting is reflected in high scores on all of the items. Consequently, we cannot rely on traditional tests of unidimensionality as evidence of construct validity (Diamantopoulos and Winklhofer 2001; Bollen and Lennox 1991). Instead, it is important to demonstrate that our items comprehensively cover all facets of the underlying construct (Bisbe et al. 2007). While we acknowledge that the list of items (1)–(5) is not exhaustive, we note that the list closely parallels issues that emerged during our field interviews. In particular, we have encountered (at least once) the following specific instances of misreporting: capitalizing investments that should be expensed; postponing the booking of a substantial provision for future losses; underestimating budgeted profit; overestimating profit forecasts; presenting an overly optimistic investment proposal; re-labeling expenses as non-operating items; and not following internal definitions of margins.

Role Conflict and Role Ambiguity

To measure role conflict and role ambiguity, we rely on the instruments developed by Rizzo et al. (1970) that have been used in most studies examining these constructs (Tubre and Collins 2000). The role conflict scale consists of eight items. These items ask controllers to indicate on a seven-point Likert scale to what extent they agree with the following eight statements: (1) I have to do things that should be done differently; (2) I receive an assignment without the manpower to complete it; (3) I have to buck a rule or policy in order to carry out an assignment; (4) I work with two or more groups of people who operate quite differently; (5) I receive incompatible requests from two or more people; (6) I do things that are apt to be accepted by one person and not accepted by others; (7) I receive an assignment without adequate resources and materials to execute it; and (8) I work on unnecessary things. Role ambiguity is measured with the six following statements: (a) I feel certain about how much authority I have; (b) It is clear what the goals are for my job; (c) I know that I have divided my time properly over different tasks; (d) I know what my responsibilities are; (e) I know exactly what is expected of me; and (f) Explanation is clear for what has to be done.

Based on prior literature, we expected to find two factors explaining variance of the 14 items measuring role conflict and role ambiguity. Nevertheless, a CFA rejects a two-factor solution even after modifications improving model fit. This is largely because the role conflict items load on more than one factor. In particular, role conflict items (3)–(7) load on a factor unrelated to items (1), (2), and (8). In retrospect, it is perhaps not surprising that controllers, who unlike most other organizational participants always have at least two bosses, attach a different meaning to items (3)–(7), which largely reflect conflicting demands of different groups of people. In fact, role conflict due to having different bosses is what we intend to measure in our study. Therefore, we calculate our proxy for role conflict (*ROLE CONFLICT*) as an equally weighted average of scores on the role conflict items (3)–(7).[8] There is support for unidimensionality of the five-item construct ($p = .97$, RMSEA $= .00$, NNFI $= 1.07$); reliability is somewhat low but still acceptable ($\alpha = .67$).

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[8] As a robustness test, we consider measures based on all eight role conflict and all six role ambiguity items. Our results from Figure 2 remain qualitatively unchanged.

Consistent with prior literature, we find that (after dropping item (e)) the role ambiguity items load on a single factor that is distinct from role conflict. Thus, we calculate our proxy for role ambiguity (*ROLE AMBIGUITY*) as an equally weighted average of reverse-coded responses on the role ambiguity items (a)–(d) and (f). There is support for unidimensionality of the five-item construct ($p = .36$, RMSEA $= .01$, NNFI $= 1.00$); reliability is satisfactory ($\alpha = .75$).

Control Variables

We also use several control variables (calculated as equally weighted averages of responses on items widely used in prior literature):

Environmental uncertainty: Our measure is based on the five environmental uncertainty items of Govindarajan (1984). Specifically, we ask respondents to indicate on a five-point Likert scale the predictability of their BU's clients, competitors, technology, suppliers, and government regulations.

Organizational technology: We measure task variability and task analyzability, the two key dimensions along which different technologies vary (Perrow 1967). Specifically, we adapt four task-variability items and four task-analyzability items from the widely used instrument of Withey et al. (1983).

Decentralization: Our measure of decentralization is based on the instrument of Abernethy et al. (2004). Respondents indicate on five-point Likert scales how much influence BU management has with regard to strategic, investment, marketing, internal processes, and human resource decisions.

Interdependencies: To capture the extent of intrafirm interdependencies, we rely on two items from Keating (1998). Respondents answer the following two questions using five-point Likert scales: (1) To what extent do your unit's actions impact on work carried out in other units of the same company? (2) To what extent do actions of other units in the company impact on the work carried out in your unit?

Finally, we control for respondents' tenure as BU controllers, BU size (using the log of the number of employees), relative size (BU number of employees divided by company number of employees) and we include a dummy variable for BUs within publicly listed companies.

IV. RESULTS

Descriptive Statistics

Table 4 provides descriptive information on characteristics of our respondents and the BUs and companies for which they work. Based on sample medians, the typical respondent is 36 years old, has 6.5 years experience in various controller functions, and has been in his/her present function for two years (untabulated results show that 80 percent of respondents are male, 48 percent hold the Dutch equivalent of a CPA title, and 54 percent are CMAs). The median BU size as measured by the number of employees is 400 and our sample BUs are often part of large companies as indicated by the median company size of 16,000 employees (the median BU employs only about 2 percent of total company employees). Manufacturing (29 percent) and financial services (27 percent) are the most common industry classifications in our sample.

Table 4 also presents descriptive information on our main and control variables. Our measure of the emphasis on the functional responsibility (*FUNCTIONAL*) has a high standard deviation and a mean somewhat lower than the midpoint of the scale (in part due to

TABLE 4
Descriptive Statistics

	n	Mean	S.D.	Min.	Median	Max.
Respondent characteristics						
Age	133	37.48	5.70	28.00	36.00	55.00
Tenure in company	134	5.76	5.58	1.00	4.00	35.00
Tenure in business unit	134	3.13	2.54	0.30	2.50	16.00
Tenure as BU controller	134	2.66	1.94	0.30	2.00	14.00
Experience as controller	134	7.53	4.76	1.00	6.50	30.00
Company and BU characteristics						
Number of BU employees	134	1,278	3,330	3.00	400	33,000
No. of company employees	134	41,517	67,544	250	16,000	400,000
Relative size	134	0.09	0.14	0.00	0.02	0.83
Main variables						
FUNCTIONAL	134	2.73	1.31	1.00	2.50	5.00
DMSUPPORT	134	3.83	0.71	1.00	3.80	5.00
MISREPORT	134	1.95	0.57	1.00	1.85	3.80
ROLE CONFLICT	134	3.26	1.00	1.00	3.20	5.60
ROLE AMBIGUITY	134	2.72	0.86	1.00	2.60	6.00
Additional control variables						
TASK ANALYZABILITY	134	3.47	0.78	1.50	3.50	5.00
TASK VARIABILITY	134	3.18	0.92	1.00	3.25	5.00
INTERDEPENDENCIES	134	3.01	1.05	1.00	3.00	5.00
UNCERTAINTY	134	3.00	0.54	2.00	3.00	4.00
DECENTRALIZATION	134	2.55	0.75	1.00	2.60	4.60

FUNCTIONAL = emphasis on the functional responsibility of BU controllers;
DMSUPPORT = BU controllers' support of local decision making;
MISREPORT = data misreporting;
ROLE CONFLICT = measure of distress due to simultaneous occurrence of two or more sets of pressures;
ROLE AMBIGUITY = measure of distress due to unclear job demands;
TASK ANALYZABILITY = a dimension describing an organization's technology;
TASK VARIABILITY = another dimension describing an organization's technology;
INTERDEPENDENCIES = interdependence of BU's within a company;
UNCERTAINTY = environmental uncertainty; and
DECENTRALIZATION = delegation of decision rights to BU level.

many respondents who indicate that functional superiors have almost no influence on controller hiring, evaluation, etc.; *FUNCTIONAL* is 1.00 for about 16 percent of the sample). Consistent with our qualitative data, we find that *DMSUPPORT* is skewed to the right because local decision-making support is important on average, while *MISREPORT* is skewed to the left because data misreporting is often viewed as unacceptable. The averages of *ROLE CONFLICT* and *ROLE AMBIGUITY* are close to the midpoint of our scales and both measures exhibit a great deal of variation.

Table 5 reports bivariate correlations among our main measures. Consistent with our predictions, we find that the emphasis on the functional responsibility of BU controllers is

TABLE 5
Bivariate Associations
(n = 134)

	1.	**2.**	**3.**	**4.**	**5.**
1. *FUNCTIONAL*	1.71	−0.28	0.09	0.23	0.28
2. *DMSUPPORT*	−0.30**	0.51	−0.02	0.03	−0.10
3. *MISREPORT*	0.12	−0.05	0.33	0.21	0.14
4. *ROLE CONFLICT*	0.17**	0.04	0.36**	1.01	0.31
5. *ROLE AMBIGUITY*	0.25**	−0.16	0.29**	0.36**	0.73

** Refers to significance at the 0.01 level (two-tailed). Pearson correlations are reported below the diagonal, variances and covariances are reported on and above the diagonal.
For variable definitions see Table 4.

negatively associated with local decision-making support and positively associated both with role conflict and role ambiguity. Both our role stress variables are in turn positively associated with data misreporting.

Hypotheses Tests

To test our theory, we need to estimate a path model as summarized in Figure 1.[9] Our theory discussed in Section II implies that a high emphasis on the functional responsibility of BU controllers compromises their ability to support local decision-making (H1). Further, H2 predicts a *direct negative effect* of the emphasis on the functional responsibility of BU controllers on data misreporting. In contrast, the remaining two hypotheses predict an *indirect positive effect*—high emphasis on the functional responsibility of BU controllers is expected to increase role conflict and role ambiguity (H3) which should in turn increase data misreporting (H4).

The results of estimating our path model are summarized in Figure 2. We find a good fit (p = .34, RMSEA = 0.03, NNFI = .98) and coefficient estimates largely consistent with our hypotheses. First, as predicted by H1, *FUNCTIONAL* is negatively associated with *DMSUPPORT* (p = .00) suggesting that greater emphasis on the functional responsibility of BU controllers reduces their ability to support local decision-making. We can reject the alternative view that there are significant complementarities between functional responsibilities of BU controllers and their support of local decision-making.

Second, there is little evidence that the emphasis on the functional responsibility of BU controllers directly reduces data misreporting. The direct effect of *FUNCTIONAL* on *DATA MISREPORTING* is insignificant (p = 0.33). This result contrasts with the finding of Indjejikian and Matějka (2006) that the emphasis on the functional responsibility of BU controllers is negatively associated with budgetary slack. However, it is possible that the lack of a significant result in our sample is due to low power of our test.

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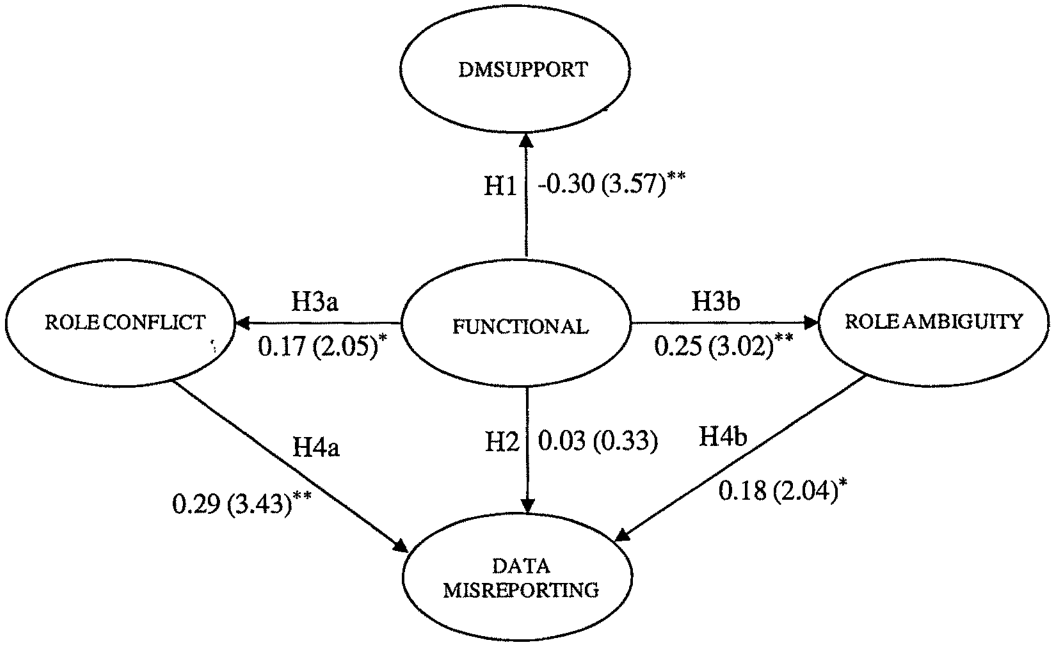
[9] Coefficients in a path model are standardized regression coefficients showing the direct effect of an independent variable on a dependent variable in the model (controlling for other independent variables). Our estimation (using LISREL 8.52) minimizes the discrepancy between sample covariances (of observed item scores) and covariances predicted by a theoretical model. To assess the goodness of fit in our estimations, we present statistics commonly used in the literature (*cf.*, Bollen 1989): (1) p-value associated with the χ^2 statistic testing for the difference between observed and theoretical covariances structures (an insignificant difference suggests a good fit), (2) root mean square error of approximation (RMSEA) measuring discrepancy per degree of freedom (values of 0.05 or less typically indicate a close fit), and (3) non-normed fit index (NNFI) for which values close to one indicate good fit.

FIGURE 2
Structural Model and Parameter Estimates (Path Analysis)



*, ** Refer to significance at the 0.05 and 0.01 levels (two-tailed), respectively.

Displayed are standardized coefficient values and their t-values in brackets. Goodness-of-fit statistics: $\chi^2 = 3.34$, d.f. = 3, p = 0.34, RMSEA = 0.029, NNFI = 0.98, n = 134. The model allows for correlation between *ROLE CONFLICT* and *ROLE AMBIGUITY*.

For variable definitions see Table 4.

Third, we find that *FUNCTIONAL* is positively associated both with role conflict and role ambiguity (p = .04 and p = .00, respectively) as predicted by H3. These results cast doubt on the view of BU controllers as impersonal information providers with no preferences regarding the emphasis on their dual responsibilities. Instead, the results are consistent with our qualitative data and support the notion that role conflict and role ambiguity arise as functional superiors seek more control over BU controllers' activities while BU controllers derive satisfaction primarily from their work with local managers.

Fourth, our results also support H4 in that role conflict and role ambiguity are positively associated with *DATA MISREPORTING* (p = .00 and p = .04). This is consistent with existing empirical evidence suggesting that role conflict and role ambiguity lead to dysfunctional behavior (Grover and Hui 1994; Grover 1993). More importantly, these results suggest that BU controllers, a key accounting institution designed to prevent misreporting, are not immune to the adverse effects of role stress.

Combined, our results contrast with the widely held view that greater emphasis on the functional responsibility of BU controllers improves the quality of financial reporting. Not only do we fail to find a significant decrease in data misreporting as a direct consequence of increasing *FUNCTIONAL*, we actually find a significant indirect effect in the opposite direction ($p = .02$).[10] Specifically, greater emphasis on the functional role goes together with role conflict and role ambiguity both of which give rise to data misreporting as a dysfunctional way to deal with role stress.

Additional Evidence and Robustness

The results presented above do not control for organizational and environmental differences to economize on the number of estimated coefficients (which is desirable given our sample size). However, we verify that controlling for various organizational and environmental characteristics does not affect our results. Specifically, we regress all five variables in our model on the control variables discussed at the end of Section III. We use the error terms from these regressions as alternative measures that filter out common effects of the control variables. We obtain qualitatively similar results.

Next, we address measurement error issues inherent in our data. In the analysis presented so far, we calculate our variables as averages of multiple observed items. Although this procedure reduces measurement error, it does not incorporate information about measurement error in the estimation of the structural model. Therefore, we also consider a structural equation model estimating both the measurement and structural parts of our model simultaneously. Figure 3 presents SEM estimates of the structural part of our model. The downside of this approach is the large number of parameters that have to be estimated simultaneously, which is critical given our limited sample size. Thus, the results below have to be interpreted with caution.

Overall, we find an acceptable fit of the SEM model ($p = .05$, RMSEA = 0.04, NNFI = .96). The results are similar to those presented in Figure 2, except that the path from role ambiguity to data misreporting is no longer significant ($p = .19$). Still, we do find that *FUNCTIONAL* has a significantly positive indirect effect on data misreporting via role conflict and role ambiguity ($p = .03$). This suggests that our main conclusions are robust to alternative ways of dealing with measurement error issues.

V. DISCUSSION AND CONCLUSIONS

Our study examines how BU controllers cope with their dual job demands. We find that the emphasis on the functional responsibility of controllers is negatively associated with their ability to support local decision-making. The management accounting literature has long argued that accounting institutions supporting primarily internal decision-making are not useful for control purposes and *vice versa*. However, there is only limited evidence on how this general trade-off affects the work of BU controllers who are important safeguards of the integrity of financial reporting.

Moreover, our survey evidence shows that a greater emphasis on the functional responsibility of controllers is associated with higher levels of role conflict and role ambiguity (two types of distress arising due to simultaneous occurrence of two or more sets of pressures or unclear job demands). Our qualitative data shed some light on this finding. Local and functional superiors of controllers as well as controllers themselves view their responsibility to support local decision-making as the primary one. Consequently, they are willing to compromise functional duties within "some range." Increasing the emphasis on the

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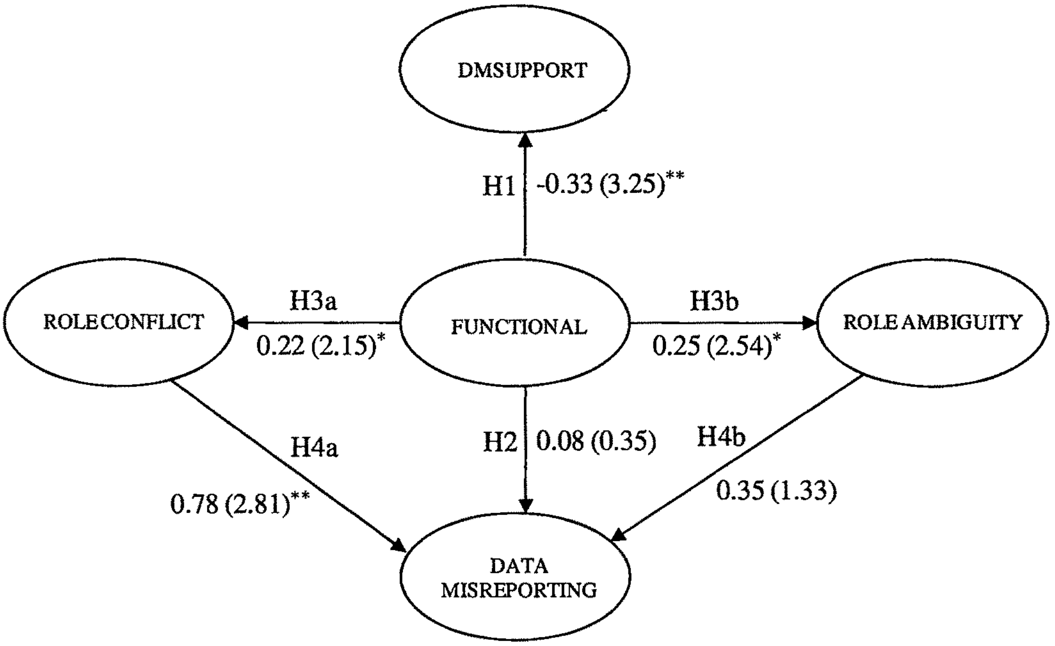
[10] Calculated as the sum of the of the indirect effect via role conflict (which is equal to the product of the coefficients on its path, (i.e., $0.17 \cdot 0.29$ as reported in Figure 2) and the indirect effect via role ambiguity ($0.25 \cdot 0.18$). The total effect, calculated as the sum of this indirect effect and the direct effect (which is the individual path coefficient in Figure 2), is positive but only marginally significant ($p = 0.16$).

FIGURE 3
Structural Model and Parameter Estimates (SEM Model)



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*, ** Refers to significance at the 0.05 and 0.01 levels (two-tailed), respectively.

Displayed are standardized coefficient values and their t-values in brackets. Parameters relating to the measurement model are omitted. Goodness-of-fit statistics: $\chi^2 = 194.2$, d.f. = 164, p = 0.05, RMSEA = 0.037, NNFI = 0.96, n = 134. The model allows for correlation between *ROLE CONFLICT* and *ROLE AMBIGUITY*.

Consistent with the discussion of measurement properties in Section III, *FUNCTIONAL, DMSUPPORT, ROLE CONFLICT,* and *ROLE AMBIGUITY* are modeled as latent variables reflected in 4, 5, 5, and 5 items, respectively. *DATA MISREPORTING* is modeled as an observed variable (with values equal to the average of its 10 items) because it is not expected to be unidimensional.

For variable definitions see Table 4.

functional responsibility, however, reduces this "range" and makes it more difficult for controllers to cope with their dual responsibilities and satisfy conflicting demands of both their functional superiors and local management (San Miguel and Govindarajan 1984; Sathe 1982).

We also find that role conflict and role ambiguity as perceived by controllers is associated with data misreporting at the local level. This is consistent with prior organizational behavior literature that has established a link between role conflict, role ambiguity and dysfunctional behavior such as absenteeism, poor productivity, or lying (e.g., Lau et al. 2003; Grover 1993; Jackson and Schuler 1985). While there is prior evidence that role stress makes tax accountants less sensitive to ethical issues (Yetmar and Eastman 2000), our study is the first (we are aware of) to show that BU controllers are not immune to the adverse effects of role stress that can, therefore, adversely affect the quality of financial reporting.

Combined, these results suggest that greater emphasis on the functional responsibility of BU controllers can indirectly lead to an increase in data misreporting. By acknowledging the existence of this indirect effect, our study goes against the widely held belief in the existing literature and in practice that emphasis on functional duties of controllers alleviates data misreporting issues. To the extent that greater emphasis on their functional duties goes together with role conflict and role ambiguity, controllers could respond with greater tolerance of misreporting as a dysfunctional way to deal with role stress.

Our conclusions are subject to several limitations. Our measure of data misreporting only indirectly reflects whether actual misreporting occurs since it is notoriously difficult to construct direct proxies based on survey responses (given data constraints, we also cannot assess psychometric properties of the measure). Similarly, our measure of the emphasis on the functional responsibility of BU controllers does not directly capture what type of tasks controllers actually perform. We acknowledge that other organizational design choices (e.g., the design of incentive compensation) are also likely to affect how BU controllers balance their dual tasks, yet leave such considerations for future research. Finally, we note that a larger sample would have allowed us to deal with measurement error issues more effectively. Despite these limitations, we believe that our study generates new insights about how controllers cope with their dual responsibilities—a topic that has received little attention in prior research.

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Internal Audit Quality and Earnings Management

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ABSTRACT: Internal auditors perform work that is relevant to their host entities' financial reporting processes; yet, little research attention has focused on the effects of internal auditing on companies' external financial reporting. Using a unique and previously unavailable data set, we investigate the relation between internal audit function (IAF) quality and earnings management. We measure IAF quality using a composite measure comprising six individual components of IAF quality based on SAS No. 65, which guides external auditors in assessing the quality of an IAF with respect to its role in financial reporting. Earnings management is measured using two separate proxies: (1) abnormal accruals and (2) the propensity to meet or barely beat analysts' earnings forecasts. We find evidence that IAF quality is associated with a moderation in the level of earnings management as measured by both proxies.

Keywords: *corporate governance; internal audit function; internal audit quality; earnings management; abnormal accruals; analyst forecasts.*

Data Availability: *Contact the authors. Data provided by the Institute of Internal Auditors Research Foundation are subject to restrictions.*

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I. INTRODUCTION

According to a model of corporate governance put forth by the Institute of Internal Auditors (IIA), an effective internal audit function (IAF) is one of four cornerstones of corporate governance, along with the audit committee of the board of directors, executive management, and the external auditor (IIA 2005a).[1] Internal auditors provide a variety of services within their host organizations, including audits of financial records and processes. To date, however, little research has examined how internal auditing might affect the quality of a company's external financial reporting. In this paper, we examine whether a high-quality IAF is associated with lower levels of earnings management. In so doing, we build on an important stream of research in accounting that examines the impact of various components of corporate governance on financial reporting.

Though little rigorous evidence exists, there is good reason to believe that internal auditing plays an important role in external financial reporting. For example, previous research has shown that management's forecasts are more biased when misrepresentation is relatively difficult to detect (Rogers and Stocken 2005) and that management's communications are more likely to be biased when they are not verified by a third party (Schwartz and Young 2002). Further, year-end financial audit work by external auditors appears to play a role in moderating earnings management by minimizing managers' opportunities to manage earnings in the fourth quarter (Brown and Pinello 2007). High-quality internal auditors serve as an additional third-party monitor of management's actions on a year-round basis; thus, we posit that as a company's IAF improves in quality, it is more likely to serve as a credible detection and deterrent mechanism that moderates earnings management.[2]

External auditing standards explicitly recognize the potential relevance of internal auditing to the financial reporting process (AICPA 1997; PCAOB 2007a). In the audit risk model, a high-quality IAF can reduce overall control risk, and the work of the IAF can be relied upon to reduce detection risk to the extent that specific indicators of IAF quality are present (AICPA 1997; PCAOB 2007a). Recent audit regulation permits and even encourages external auditors to "use the work of others to a greater extent when the work is performed by sufficiently competent and objective persons" (PCAOB 2007b, 13). Consistent with these arguments, Felix et al. (2001) and Prawitt et al. (2009) find evidence that external audit fees are lower for companies that employ higher-quality IAFs, and Anderson et al. (1993) report an inverse relation between external audit fees and internal audit budgets.

We build on the limited research that examines the role of internal auditing in external financial reporting by providing new evidence about the ability of the IAF to moderate earnings management. We examine the relation between IAF quality and earnings management by combining publicly available data with a unique and previously unavailable data set from the IIA. Using data from 218 publicly traded companies for fiscal years 2000 through 2005 (528 firm-year responses), we test our prediction that a composite measure of IAF quality is negatively associated with two different proxies for earnings management.

We are guided in the formulation of our composite IAF quality measure by external auditing standards. These standards provide detailed guidance to external auditors in evaluating the individual components of IAF quality before relying on internal auditors' work in the course of a financial statement audit (AICPA 1997).[3] The specific components that we use to create our internal audit quality measure include the average experience of the internal auditors, the percentage of internal auditors that are professionally certified (CIA or CPA), the amount of time spent training during the year, the relative focus of the IAF on financial audit work, the reporting relation of the head of the IAF, and the size of the IAF relative to its industry. We measure earnings management using two different proxies: (1) the absolute value of abnormal accruals as determined by the performance-adjusted, modified Jones model, and (2) the propensity of companies to meet versus just miss analysts' consensus earnings forecasts.

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[1] Several other models of corporate governance also include the IAF as an integral part of high-quality corporate governance (e.g., see Skousen et al. 2005; Hermanson and Rittenberg 2003; Treadway Commission 1987).

[2] The earnings management proxies we study do not necessarily reflect GAAP violations. Nevertheless, to the extent management may suffer negative consequences if their accounting choices are determined to be aggressive (e.g., reputational losses, increased scrutiny, reduction in future discretionary choices, etc.), earnings management is likely to be moderated as the quality of deterrent mechanisms, including internal auditing, increases.

We find a significant negative relation between overall IAF quality and absolute abnormal accruals. An examination of signed abnormal accruals suggests that this relation is largely driven by a moderation of negative abnormal accruals. We also find that companies that just miss analysts' expectations have IAFs that are of significantly higher quality relative to the rest of the sample. These results are robust to the inclusion of proxies for various factors that prior research identifies as being associated with abnormal accruals and for various aspects of corporate governance quality, including the quality of the audit committee, the quality of the external auditor, and management's relative power (as measured by Gompers et al.'s [2003] corporate governance index). A supplementary two-stage least-squares analysis provides additional assurance that the results for the abnormal accruals model are not attributable to potential endogeneity concerns. The results of a two-stage least-squares analysis for the miss/beat analysts' expectation model are not significant. However, on balance we conclude that the results provide evidence consistent with the idea that higher-quality IAFs are associated with lower levels of earnings management.

In Section II, we discuss the relevant literature and develop our research expectation. In Section III, we discuss the study's methodology, followed by a discussion of results in Section IV. We conclude in Section V by discussing the implications of our findings, the limitations of our study, and areas for future research.

II. LITERATURE REVIEW AND RESEARCH EXPECTATION

As a result of the highly publicized accounting scandals in recent years, the IAF has received greater attention as an important contributor to effective corporate governance and quality external financial reporting. For example, *Time* named internal auditor Cynthia Cooper its "Person of the Year" in 2002 for her discovery and reporting of the WorldCom fraud (Lacayo and Ripley 2002), and the New York Stock Exchange now requires that all listed companies maintain an IAF (NYSE 2004). The NASDAQ stock exchange does not require listed companies to have an IAF, but does encourage it as a best practice (Harrington 2004).

Though regulators and the popular press have only recently begun to recognize the importance of the IAF with respect to external financial reporting, the IAF has long been recognized in the academic literature as a key component of corporate governance with an important monitoring role (e.g., see Anderson et al. 1993), and models of corporate governance have long included the IAF (e.g., see Skousen et al. 2005; Hermanson and

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[3] Specifically, SAS No. 65, *The Auditor's Consideration of the Internal Audit Function in an Audit of Financial Statements,* indicates that external auditors must evaluate an IAF's competence, objectivity, and work performed before relying on the IAF's work. The standard also suggests specific indicators for evaluating the IAF in each of these three areas, several of which we include in our composite quality measure (AICPA 1997). PCAOB standards relating to the external auditor's reliance on work performed by the internal auditor parallel those of the AICPA (PCAOB 2007a).

Rittenberg 2003; Treadway Commission 1987). As mentioned previously, the IIA model of corporate governance suggests that the IAF is one of four cornerstones of high-quality corporate governance, together with the audit committee, the external auditor, and management. Of these four cornerstones, only management and the IAF typically have day-to-day, year-round dealings with the company. A significant body of empirical accounting research suggests that management is often willing to act opportunistically to the detriment of key stakeholders. The IAF is often the party primarily responsible for the day-to-day monitoring of management's actions, including those relating to external financial reporting.

Though the IAF has the potential to significantly influence a company's external financial reporting, this potential relation has not been subjected to a great deal of rigorous examination due to the absence of relevant data. We are aware of only one prior effort to use archival data to examine the effect of the IAF on earnings management. Using a sample of 434 Australian companies, Davidson et al. (2005) find no evidence that the presence (versus absence) of an IAF is associated with a lower level of earnings management.

Expanding on the scope of the Davidson et al. (2005) study is important for several reasons. First, using the proxy of IAF presence versus absence does not generalize well to most large- or medium-sized, publicly traded companies in the United States. The requirements of some stock exchanges, the significant SOX compliance requirements, and the increased focus on corporate governance have resulted in nearly all large- and medium-sized publicly traded companies in the United States having an IAF of some kind. With respect to these companies, the effect of the *quality* of the IAF on financial reporting is a more relevant question than the effect of the *presence* of an IAF. Second, the lack of statistically significant evidence of an association between the presence of the IAF and earnings management may be attributable to the facts that companies invest in and use their IAFs quite differently and that IAFs vary widely in quality. A dichotomous variable indicating the presence or absence of an IAF cannot account for the significant variation in the nature and focus of the companies' investments in their IAF and is thus an inherently noisy measure.

Although extant archival studies provide little evidence concerning the relation between internal auditing and earnings management, experimental studies provide some evidence that an effective IAF can deter aggressive financial reporting. Schneider and Wilner (1990) find that both internal and external auditing have a deterrent effect on financial reporting irregularities and that the effects of internal auditing are similar to those of external auditing. Asare et al. (2008) demonstrate that internal auditors are sensitive to management's incentive to misreport financial information and that they increase budgeted work hours when management has a high incentive to misreport. These experimental studies suggest that internal auditing can play a role in mitigating aggressive accounting behavior by management. It is reasonable to expect, however, that the degree of this deterrent effect will depend on the quality of the IAF, which is rooted in the IAF's competence and in its independence from management.

As mentioned previously, research has shown that management is less biased when their bias is likely to be perceived by others (e.g., see Rogers and Stocken 2005; Schwartz and Young 2002). Hunton et al. (2006) demonstrate that management is less likely to manage earnings when there is greater transparency. Brown and Pinello (2007) show that year-end financial audit work by external auditors limits managers' opportunities to manage earnings in the fourth quarter. A high-quality IAF can provide greater transparency in the organization through monitoring and make it more likely that bias in management's judgments will be detected. Thus, a high-quality IAF focused on improving financial reporting can serve to detect and deter opportunistic or biased choices. Based on these arguments, our expectation is that a high-quality IAF is associated with less earnings management.

III. METHODOLOGY AND MODEL SPECIFICATION

The purpose of this study is to test whether IAF quality is negatively associated with earnings management. Accordingly, we develop a composite measure of internal audit quality from a proprietary archive developed and maintained by the IIA, which contains data relating to hundreds of different entities and their IAFs. We correlate this measure with two independent measures of earnings management: (1) performance-adjusted abnormal accruals (Kothari et al. 2005) and (2) a measure capturing whether firms beat or miss analysts' forecasts. In the next sections, we discuss the data and our sample, our measures of internal audit quality, the abnormal accrual model, and the beat/miss analysts' forecast model.

Data and Sample

The data we use to measure IAF quality comes from the IIA GAIN database, which is a compilation of survey responses from chief audit executives (CAEs) associated with IIA member organizations.[4] The IIA does not identify the responding organizations in data that they share for benchmarking or research purposes. Therefore, with permission, we performed a match of several self-reported fields in the survey with data items in Compustat in order to obtain measures relating to earnings management as well as relevant control variables.

To match the GAIN data with data in Compustat, we matched self-reported total assets, total revenues, and operating industry in the IIA database with similar fields in the Compustat database.[5] After identifying firms and procuring necessary data from the Compustat and Risk Metrics databases (described in more detail later in the paper), we obtained sufficient data to estimate our abnormal accrual models for 528 firm-year observations (218 unique companies) for fiscal years 2000 to 2005. For purposes of the beat/miss analysts' forecast model, we have 571 firm-year observations (244 unique companies) for the same time period. Table 1 includes an explanation of how we arrived at our final samples.

Measures of Internal Audit Quality

To test our research expectation, we derive a composite measure of internal audit quality based on external auditing standards. External auditing standards define IAF quality in terms

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[4] The GAIN survey is approximately 30 pages long and covers a wide range of institutions (e.g., publicly traded companies, private companies, educational institutions, divisions within companies, and educational and governmental institutions) and various aspects of internal audit practice. Its primary purpose is to provide benchmarking data to participating firms. A copy of the most recent survey can be found by searching for "GAIN benchmarking" on the IIA's website (http://www.theiia.org). The survey has changed slightly from year to year; however, all of the questions included in this study were unchanged from 2000 to 2005.

[5] Every firm-year match in our sample represents a unique combination of assets, revenues, and industry identifiers reported in both the GAIN and Compustat databases. In other words, there were no instances where more than one firm from Compustat matched the combination of the three identifiers. In addition, the IIA kindly provided names for a subset of firms in the GAIN database, which we were able to use to validate our matching process. Our matches were consistent in every case with the information provided by the IIA.

TABLE 1
Derivation of Sample

Description	Sample Size for Abnormal Accruals Analyses	Sample Size for Beat/Miss Analyst Forecast
Firm-Year Observations In Internal Auditing Database (2000 to 2005)	4,178	4,178
Less Observations with No Reported Data in Compustat	(3,098)	(3,098)
Less Observations with No Abnormal Accrual Measure (including financial service firms for abnormal accrual analyses)	(225)	—
Less Observations with Missing Data Items for Model Estimation	(285)	(460)
Less Observations where IAF Reported Values Are Nonsensical[a]	(42)	(49)
Firm-Years Available for Final Sample	528	571

[a] IAF values deemed extreme or nonsensical include percentage of certified internal auditors (CIA or CPA) greater than 100 percent, reported average overall internal audit experience levels greater than 30 years or listed as 0 years, time spent on financial audit work greater than 100 percent, and time spent training greater than 160 hours.

The final abnormal accruals sample of 528 firm-years is from 218 different companies from 44 different two-digit SIC code industries. The final beat/miss analyst forecast sample of 571 firm-years are from 244 different companies from 50 different two-digit SIC code industries.

of the competence and objectivity of the internal auditors, together with the degree to which their work is relevant to the financial statement audit (AICPA 1997; PCAOB 2007a). Our composite measure of audit quality is based on six individual characteristics specifically indicated by external auditing standards relating to the competence and objectivity of and the work performed by the internal auditors.[6]

Internal auditors are expected to "possess the knowledge, skills, and other competencies needed to perform their individual responsibilities" (IIA 2005b, §1210). The more competent the internal auditors, the more likely they are to understand the factors leading to and the indicators of management bias in accounting accruals and how it can be moderated. In addition, management has less incentive to aggressively manage earnings if they have reason to believe that a competent IAF is monitoring their accounting choices. SAS No. 65 stipulates that the external auditor should consider several specific factors in evaluating competence, including professional experience, professional certifications, and training (AICPA 1997). We use three variables to proxy for these factors, *Experience*, *Certification*, and *Training*. *Experience* is the average number of years of internal auditing experience of the internal auditors in the IAF. *Certification* is the percentage of internal auditors in the IAF who have the CIA or CPA certification. *Training* is the average number of hours of training the internal auditors completed during the year.[7]

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[6] Due to a critical lack of detailed data, previous studies have employed various proxies for IAF contribution. These previous proxies are, at best, indirect measures of an IAF's potential contribution relative to financial reporting and each measures the IAF contribution using a single variable, usually relating to the presence, size, or extent of investment in an IAF. Further, other studies find them to be relatively poor proxies for IAF quality (Prawitt et al. 2009). By contrast, we measure IAF quality in terms of a composite variable that captures multiple characteristics specified by external auditing standards as indicators of a high-quality IAF. This improved measure of IAF quality is made possible by access to the IIA's unique GAIN data archive.

SAS No. 65 also requires that the external auditor evaluate the objectivity of a client's IAF in determining an appropriate level of reliance (AICPA 1997). Objectivity is important to an internal auditor's ability to play a significant role in affecting the quality of a company's financial reporting. The more objective the IAF, the more likely it will be to report evidence of earnings management should it be discovered. Also, an objective IAF would be less likely to be "steered away" from working in areas in which management was managing earnings through manipulation of accruals. According to SAS No. 65 and relevant IIA standards, the primary organizational aspect of IAF objectivity revolves around the reporting status of the head of the IAF, or chief audit executive (CAE). To proxy for objectivity, we use *CAEAC*—a dummy variable indicating whether the head of the IAF reports to the audit committee ($CAEAC = 1$) or to management ($CAEAC = 0$).[8]

Because internal auditing is a relatively unregulated activity, the focus of the IAF varies widely across organizations. IAFs may perform, among other tasks, financial, operational, fraud, control, compliance, or systems audits as well as internal consulting projects for management (e.g., see Anderson 1996; Klinkerman 1996; Thevenin 1997). SAS No. 65 requires the external auditor to evaluate the nature of the work performed by the IAF in terms of its relevance to financial reporting as it concerns the work of the external auditor. To proxy for the degree of IAF focus on financial work, we include *TimeFin*, which is the percentage of internal audit time spent performing financial audits.[9]

Finally, we include *IASize* as an overall measure of the entity's investment in the IAF. IAFs that are relatively well funded should have greater ability to monitor a company's transactions. The greater the monitoring ability of the IAF, the more able it will be to detect and deter management bias or opportunism. *IASize* is computed by subtracting the average dollar amount spent on internal auditing for the industry, divided by the average total assets of the industry, from the dollar amount spent on internal auditing per company, divided by the company's total assets. The result of this calculation is then divided by the average dollar amount spent on internal auditing for the industry, divided by the average total assets of the industry.[10]

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[7] Other factors specified in the standard for the external auditor to evaluate with respect to internal auditor competence are: audit policies, programs, and procedures; internal auditors' performance; quality of working paper documentation, reports, and recommendations; practices regarding assignment of internal auditors; and practices regarding supervision and review. Measures relating to these other factors are not available in the IIA's GAIN database and are not otherwise publicly available.

[8] SAS No. 65 specifies two elements in evaluating an IAF's objectivity: (1) the organizational status of the internal auditor responsible for the IAF and (2) the policies that the IAF has in place to maintain internal auditors' objectivity with respect to areas audited. The IIA GAIN database has information about the organizational status of the CAE (i.e., reporting relationship), but does not contain detailed information regarding IAF policies to maintain objectivity.

[9] In practice, the external auditor would also make an assessment of the quality of the work performed by the internal auditor that is specifically relevant to the external auditor's opinion. The IIA GAIN database includes data that is germane to the general nature and quality of the IAF, but does not include quality assessments of specific work performed relative to particular engagements.

To create an overall, composite measure of internal audit quality, we dichotomize each variable by assigning a value of 1 to the variable if it is above the median of our sample for that variable, and 0 if it is below the sample median.[11] We then sum the scores of the individual quality components to create *IAQuality*, a composite measure of internal audit quality potentially ranging from 0 to 6. Larger *IAQuality* scores indicate IAFs that are of higher quality.[12]

Table 2, Panel A presents descriptive statistics relating to *IAQuality* and the individual quality components used to create *IAQuality*. Of note in Panel A is the significant variation in *IAQuality* and in each of the individual components, which are combined to provide an overall quality measure. The variable ranges from 0 to 5 with first and third quartile scores of 2 and 3, respectively. Despite the fact that included companies self-select into the IIA's GAIN database, these descriptive data suggest that IAFs representing a reasonable degree of variation in quality responded to the survey.

Abnormal Accrual Model

To measure abnormal accruals, we use the performance-adjusted cross-sectional variation of the modified Jones model (Dechow et al. 1995) as reported by Kothari et al. (2005). Kothari et al.'s (2005) simulation analysis demonstrates that including an intercept term and adjusting for performance reduces noise and increases the accuracy of the abnormal accrual measure. Accordingly, our estimate of abnormal accruals is the residual from the following regression:

$$\frac{TA_{it}}{A_{it-1}} = \beta_0 + \beta_1\left(\frac{1}{A_{it-1}}\right) + \beta_2\left(\frac{\Delta REV_{it} - \Delta AR_{it}}{A_{it-1}}\right) + \beta_3\left(\frac{PPE_{it}}{A_{it-1}}\right) + \beta_4\left(\frac{NI_{it}}{A_{it-1}}\right) + \varepsilon_{it}.$$

We use the model to separately estimate an abnormal accrual by industry (two-digit SIC code) and year for all firms in Compustat from 2000 to 2005.[13] We require ten firm-year

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[10] In formula form this calculation is as follows:

$$\frac{(\text{Firm \$IAF/Firm \$TA}) - (\text{Ind. \$IAF/Ind. \$TA})}{(\text{Ind. \$IAF/Ind. \$TA})}$$

This number is computed for all firms in the GAIN database, regardless of whether these firms could be matched with Compustat. Therefore, we used self-reported industry codings from the GAIN database. The GAIN database separates companies into 13 different industry groupings including: Agriculture/Mining/Construction, Petroleum, Wholesale/Retail, Chemical/Drug, Manufacturing, Educational Institutions, Transportation, Communications/Telecommunications, Banking/Financial, Utilities/Gas/Electric, Insurance, Services, and Government.

[11] We assign one point to the *IASize* quality component if it is positive (above the industry average) and no points if it is negative (below the industry average).

[12] Previous external audit fee research demonstrates that the external audit fee is significantly lower for firms that have a high-quality IAF (e.g., see Felix et al. 2001).

[13] Consistent with prior research using abnormal accruals as a proxy for earnings management, we exclude financial institutions (SIC 6000 to 6900) from our analysis as these companies have special accounting practices that make estimating abnormal accruals difficult.

TABLE 2
Descriptive Statistics and Correlations

Panel A: Descriptive Statistics of Internal Audit Characteristics

Variable	Mean	Std. Dev.	Min	P25	Median	P75	Max
IAQuality	2.42	1.15	0.00	2.00	2.00	3.00	5.00
Experience	6.35	4.18	1.00	3.00	6.00	8.00	28.00
Certification	0.59	0.30	0.00	0.40	0.55	0.71	2.00
CAEAC	0.69	0.46	0.00	0.00	1.00	1.00	1.00
TimeFin	31.79	20.01	0.00	12.00	33.00	48.00	80.00
Training	59.21	26.32	0.00	40.00	60.00	80.00	160.00
IA Size	0.51	1.27	−0.79	−0.27	0.14	0.78	9.71

Panel B: Descriptive Statistics of Variables Included in Our Models

Variable	Mean	Std. Dev.	Min	P25	Median	P75	Max
AbnAccr	−0.046	0.108	−0.883	−0.083	−0.022	0.013	0.325
ACEffectiveness	9.36	2.18	0.00	10.00	10.00	10.00	10.00
AuditorSpecialist	0.52	0.50	0.00	0.00	1.00	1.00	1.00
Gindex	9.87	2.34	3.00	8.00	10.00	11.00	16.00
Assets	21,425	36,478	162	3,589	11,274	26,056	370,782
Age	44.66	13.14	14.00	35.00	52.00	54.00	56.00
Leverage	0.48	0.14	0.05	0.39	0.48	0.57	1.21
Complexity	3.55	2.14	1.00	1.00	3.00	5.00	10.00
CFO	2,218	4,022	−3,657	270	828	2,198	33,764
SalesGrowth	0.10	0.33	−0.94	−0.01	0.06	0.15	4.62
MB	2.68	16.58	−280.44	1.45	2.06	3.47	115.84
CFOVolatility	538	877	7	92	223	571	6,409
ROA	0.04	0.07	−0.34	0.02	0.04	0.07	0.51
Loss	0.15	0.35	0.00	0.00	0.00	0.00	1.00
NYSE	0.92	0.27	0.00	1.00	1.00	1.00	1.00

(continued on next page)

TABLE 2 (continued)

Panel C: Spearman and Pearson Correlations (above/below the diagonal, respectively)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	15)	(16)	(17)	(18)
1 *AbnAccr*		—	—	*−0.08*	**−0.09**	**−0.16**	**0.11**	−0.07	**−0.17**	**−0.09**	0.01	0.04	**−0.20**	**0.30**	**0.13**	**0.20**	0.06	−0.07
2 *+AbnAccr*	—		—	0.00	0.03	−0.03	0.07	−0.03	−0.08	0.06	0.06	**−0.18**	**−0.20**	*0.14*	**0.17**	0.00	0.05	0.06
3 *−AbnAccr*	—	—		**0.12**	**0.11**	**0.17**	**−0.14**	0.08	**0.21**	**0.11**	0.03	−0.07	**0.22**	**−0.30**	*−0.10*	**−0.22**	−0.07	**0.11**
4 *IAQuality*	−0.07	−0.04	0.09		*−0.08*	0.02	0.01	**−0.20**	−0.03	**0.18**	*0.08*	**−0.21**	−0.01	**−0.14**	**−0.25**	**−0.09**	0.01	**0.15**
5 *ACEffectiveness*	−0.04	0.03	0.03	**−0.09**		**0.12**	0.00	**0.10**	**0.14**	0.01	−0.01	*0.07*	−0.02	−0.02	0.05	*−0.08*	0.07	0.03
6 *AuditorSpecialist*	**−0.12**	*−0.14*	*0.09*	0.02	*0.08*		**−0.15**	*0.08*	**0.13**	**0.09**	0.03	0.02	−0.05	−0.04	0.03	−0.05	*−0.08*	**0.17**
7 *Gindex*	0.03	0.03	−0.03	−0.01	0.06	**−0.14**		**−0.14**	0.04	0.03	**0.09**	**−0.10**	−0.05	0.01	−0.06	0.04	*−0.08*	0.05
8 *Assets*	*−0.07*	0.06	*0.09*	**−0.20**	*0.08*	**0.09**	**−0.09**		**0.23**	−0.03	0.08	**0.84**	−0.02	**0.18**	**0.83**	0.00	−0.04	0.05
9 *Age*	**−0.16**	0.00	**0.20**	0.07	0.04	0.04	*0.08*	**0.25**		0.04	**0.18**	**0.17**	−0.03	−0.01	**0.12**	0.04	**−0.12**	**0.28**
10 *Leverage*	0.01	0.04	−0.03	**0.16**	0.01	**0.10**	0.00	−0.03	0.05		−0.07	**−0.10**	0.00	−0.05	−0.06	**−0.19**	**0.12**	**0.19**
11 *Complexity*	0.00	**0.15**	0.03	*0.07*	−0.03	0.03	*0.08*	**0.09**	**0.22**	**−0.09**		0.03	*0.07*	−0.02	0.00	−0.04	0.04	**0.10**
12 *CFO*	**0.12**	0.07	*−0.10*	**−0.21**	0.00	**−0.09**	**−0.13**	**0.64**	0.03	**−0.22**	0.00		0.00	**0.36**	**0.76**	**0.26**	**−0.18**	0.05
13 *SalesGrowth*	**−0.11**	−0.02	**0.15**	**0.09**	−0.02	0.00	−0.02	0.00	0.00	0.05	0.05	0.00		**0.09**	**−0.10**	**0.23**	**−0.24**	0.00
14 *MB*	**−0.15**	**0.17**	**0.23**	−0.05	0.00	*0.08*	−0.02	*0.07*	*0.08*	**−0.09**	0.03	0.05	−0.02		**0.23**	**0.52**	**−0.16**	**−0.09**
15 *CFOVolatility*	**0.10**	**0.17**	−0.08	**−0.20**	0.02	−0.02	**−0.14**	**0.63**	0.03	**−0.12**	−0.01	**0.79**	−0.03	0.04		0.07	0.07	−0.01
16 *ROA*	**0.33**	0.05	**−0.37**	−0.02	−0.07	−0.05	0.03	−0.03	0.02	**−0.09**	−0.02	**0.20**	**0.12**	**−0.14**	0.03		**−0.57**	0.01
17 *Loss*	0.00	0.00	−0.01	0.02	0.06	*−0.08*	*−0.08*	−0.04	**−0.13**	**0.12**	0.03	**−0.10**	**−0.16**	0.01	0.07	**−0.54**		**−0.12**
18 *NYSE*	−0.05	0.03	0.06	**0.15**	0.02	**0.17**	0.04	**0.09**	**0.30**	**0.22**	**0.09**	**−0.11**	0.02	−0.03	−0.07	0.02	**−0.12**	

Italics indicate p-value ≤ 0.10.

Bold indicates p-value ≤ 0.05.

Correlations for *+AbnAccruals* (*−AbnAccruals*) are the correlations with only those firm-year responses that had abnormal accruals greater or less than zero. Reported descriptive statistics and correlations are for the abnormal accrual analysis sample (n = 528). Descriptive statistics are similar for the analyst's forecast sample.
See the Appendix for variable descriptions.

observations per industry to compute the abnormal accrual measure. We use the absolute value of the error term, e_{it}, as our measure of abnormal accruals. However, to gain further insights from the data, we also report our results separately for income-increasing and income-decreasing abnormal accruals (see Hribar and Nichols 2007). As seen in Panel B of Table 2, *AbnAccr* (before taking the absolute value) ranges from -0.833 to 0.325 with a mean (median) value of -0.046 (-0.022). Kothari et al. (2005) report a similar mean of -0.03 for all firms in Compustat with sufficient data to estimate abnormal accruals from 1963 to 1999. However, the maximum and minimum of our sample fall within the first and third quartiles reported by Kothari et al. (2005). This suggests that our sample is similar to the full Compustat population in terms of mean abnormal accruals, but it does not reflect as much variation in abnormal accruals as the Compustat population.

To test the relation between internal audit quality and abnormal accruals, we use the following model:[14]

$$\begin{aligned} AbnAccr = {} & \beta_0 + \beta_1 IAQuality + \beta_2 ACEffectiveness \\ & + \beta_3 AuditorSpecialist + \beta_4 Gindex + \beta_5 Assets + \beta_6 Age \\ & + \beta_7 Leverage + \beta_8 Complexity + \beta_9 CFO + \beta_{10} SalesGrowth \\ & + \beta_{11} MB + \beta_{12} CFOVolatility + \beta_{13} ROA + \beta_{14} Loss + \beta_{15} NYSE \\ & + \beta_{16-20} IndustryDummies + \beta_{21-25} YearDummies + \varepsilon. \end{aligned}$$

See the Appendix for variable descriptions.

The dependent variable, *AbnAccr*, is the absolute value of the abnormal accrual measure. We also separate *AbnAccr* into firm-year responses that are strictly positive (*+AbnAccr*) or strictly negative (*−AbnAccr*). To test our research question, we include *IAQuality* as an independent variable. As previously explained, this metric proxies for the overall quality of the IAF; we expect the measure to be negatively associated with *AbnAccr*. In addition to *IAQuality*, we include 24 control variables in our model.

Because internal audit is a component of corporate governance, we include three control variables to measure the contribution of the other three components of corporate governance (IIA 2005a). The inclusion of a measure for each component of corporate governance increases the likelihood that the coefficient on *IAQuality* represents the incremental contribution of the IAF toward the moderation of earnings management.

Previous research demonstrates that higher-quality audit committees are associated with greater internal auditor involvement, including larger budgets (Carcello et al. 2005), longer meetings, increased private access to the audit committee, and reviews of audit proposals and results (Raghunandan et al. 2001). Abbott et al. (2007) find that effective audit committees are more able to discern between the types and providers of outsourced internal audit services. Other research suggests that more effective audit committees are associated with lower levels of earnings management (e.g., see Klein 2002; Xie et al. 2003; Bédard et al. 2004). Using a larger sample and controlling for more corporate governance mechanisms, Larcker et al. (2007) find a mixed relation between proxies for corporate governance, including audit committee measures, and abnormal accruals.[15] To control for the effect the audit committee has on abnormal accruals, we include *ACEffectiveness.*

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[14] Results are qualitatively similar to those reported when we winsorize or trim the abnormal accrual estimates at the 1 percent level. Also, because we have multiple observations from the same company, we use cluster-adjusted standard errors in computing z-statistics and subsequent p-values. The point estimates of the coefficients are unaffected by this procedure.

We form *ACEffectiveness* by creating a composite score comprising ten dummy variables suggested by the Blue Ribbon Committee for Audit Committee Effectiveness (BRC 1999).[16] For each attribute the company possesses, we assign the company one point and then sum the scores to create *ACEffectiveness.*[17] As in previous research, we expect to observe a negative coefficient on *ACEffectiveness,* though the mixed evidence presented by Larcker et al. (2007) casts some doubt on this expectation.

AuditorSpecialist is a dummy variable that indicates whether the external auditor is an industry specialist auditor.[18] We define industry-specialist auditor in a similar fashion to Palmrose (1986); that is, an external audit firm is considered the industry specialist (*AuditorSpecialist* = 1) if the firm provides within-industry market share 30 percent greater than if the audit firms were to split the industry evenly among themselves. We expect to observe a negative coefficient on *AuditorSpecialist.*

Finally, to proxy for management's role in corporate governance, we include Gompers et al.'s (2003) corporate governance measure—*Gindex. Gindex* is formed by giving an organization a point for each takeover defense or lack of shareholder rights for 24 different provisions. Higher *Gindex* scores suggest that management has greater power, which suggests lower corporate governance quality. Previous research by Bergstresser and Philippon (2006) include *Gindex* in an abnormal accrual model and find no relation with abnormal accruals. However, Bergstresser and Philippon (2006) include dummy variables to represent various levels of *Gindex,* whereas we include a single continuous variable. We expect to observe a positive coefficient on the *Gindex* score.

The remaining control variables are included to control for other factors that may impact a company's abnormal accruals. Dechow and Dichev (2002) find that larger firms have larger accruals; hence, we control for firm size by including the log of total assets (*Assets*). We include *Age* because previous research has shown that accruals change based on the firm life cycle (Anthony and Ramesh 1992). Prior research has found that highly leveraged firms are associated with both income-increasing accruals (Press and Weintrop 1990) to meet debt covenant restrictions and income-decreasing accruals (DeAngelo et al. 1994) to reduce earnings for contractual renegotiations. Because we use the absolute value

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[15] Larcker et al. (2007) do not consider internal audit characteristics, external audit characteristics, or management characteristics in testing the relation between abnormal accruals and corporate governance, but instead, using proprietary data, focus on characteristics of the board of directors, stock ownership by executives and board members, stock ownership by institutions, stock ownership by activist holders, debt and preferred stock holdings, compensation mix variables, and anti-takeover devices.

[16] The ten questions relate to independence, financial literacy skills, assessing the committee's charter, fulfilling the charter, accountability for auditor relations, relation with external auditor, discussion of accounting, disclosure of reviews, and reviewing of quarterly reports. Qualitatively similar results are obtained if we average the ten items or create a single composite measure using factor analysis. The items are highly correlated and therefore we do not include all the items individually in the same regression.

[17] A possible alternative measure for controlling for audit committee quality is the presence or absence of an audit committee. We cannot use this proxy as all but three firm-year responses in our sample indicate presence of an audit committee.

[18] An alternative measure of auditor quality is Big N versus non-Big N auditor. Previous research suggests that Big N accounting firms tend to be more conservative in their auditing approach than smaller firms (Becker et al. 1998; Francis et al. 1999; Francis and Krishnan 1999); therefore, earnings management is likely to be lower for companies audited by a Big N auditor. We do not include a measure for Big N auditor because all but eight firm-year observations in our sample indicate an audit by a Big N auditor.

of abnormal accruals as our primary dependent measure, we expect *Leverage* to control for both effects and therefore to have a negative sign as demonstrated in prior research (Frankel et al. 2002). We include *Complexity*, which measures the number of operating segments, because managers of companies that have more complex operations may both invest in higher-quality IAFs and have greater ability to hide earnings management efforts.

To control for the possibility that our results are due to firm growth, we include *CFO* (Dechow et al. 1996), *SalesGrowth* (Menon and Williams 2004), and *MB* (Matsumoto 2002). In addition, we follow Dechow and Dichev (2002) in controlling for the volatility of cash flows by including *CFOVolatility*. Managers at firms that are struggling have increased incentives to manage earnings; therefore, we include *ROA* and *Loss*. *ROA* is computed as net income divided by assets, and *Loss* is a dichotomous variable taking the value of 1 (0) if net income is below (above) zero for the year. We include *NYSE* because firms listed on this exchange are required to have an IAF (NYSE 2004). Finally, we include *IndustryDummies* and *YearDummies* to control for differing levels of abnormal accruals by industry and year. The five industry dummy variables control for industry at the one-digit SIC level.

Beat/Miss Analysts' Forecast Model

An alternative measure of earnings management is whether management attempts to meet or beat certain earnings thresholds such as analysts' forecasts (e.g., see Brown and Caylor 2005; Burgstahler and Dichev 1997; DeGeorge et al. 1999). Brown and Caylor (2005) find evidence that meeting or beating analysts' forecasts was the most important metric managers sought to achieve from the mid-1990s to 2002. Thus, as a secondary proxy for earnings management, we consider whether companies that employ a high-quality (low-quality) IAF are more (less) likely to miss (meet or just exceed) an analysts' forecasts. We consider meeting/beating and missing analysts' forecasts for annual forecasts because our data relating to IAF quality is recorded annually.

We gather our data from Thomson Financial I/B/E/S for 2000 to 2005. Using the summary file of the I/B/E/S database, we compute whether the company made or missed analysts' forecasts for earnings by subtracting the consensus annual earnings forecast from the reported consensus forecast closest to the announcement date from actual annual earnings. The companies that are most likely to have managed (not managed) earnings are those that meet/just beat (just miss) analysts' expectations. For our tests, we therefore compare these firms against each other and against all firms that did not narrowly meet/miss analysts' expectations. To compute which firms just met/missed analyst's expectations, we divide how close the firm was to consensus forecast by price at the beginning of the year and then examine a bin that is within 0.0005 of 0.[19] This results in 42 firm-years that missed analysts' expectations, 139 firm-years that met or beat analysts' expectations, and 390 other firm-year responses.

We use the following multinomial logistic model[20] to test whether companies that just missed (met) analyst expectations are more (less) likely to have a high-quality IAF:

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[19] As an alternative to this measure, we create bins without scaling. All firm-years responses that are within \$0.02 per share (unscaled by differences in share price across observations) of analysts' expectations (either above or below) are separated into bins. The results are qualitatively similar to those reported using this unscaled measure.

[20] The multinomial logit model reports all pairwise combinations with respect to three categories of the dependent variable (i.e., just missed/just beat, just missed/all other, just beat/all other).

$$
\begin{aligned}
Above = {} & \beta_0 + \beta_1 IAQuality + \beta_2 ACEffectiveness + \beta_3 AuditorSpecialist \\
& + \beta_4 Gindex + \beta_5 Assets + \beta_6 Age + \beta_7 Leverage + \beta_8 Complexity \\
& + \beta_9 CFO + \beta_{10} SalesGrowth + \beta_{11} MB + \beta_{12} CFOVolatility \\
& + \beta_{13} ROA + \beta_{14} Loss + \beta_{15} NYSE + \beta_{16-20} IndustryDummies \\
& + \beta_{21-25} YearDummies + \varepsilon.
\end{aligned}
$$

See the Appendix for variable descriptions.

The dependent variable *Above* is an ordinal variable taking the value of 1 if the company just missed the consensus analysts' expectation, 2 if the company just met or beat analysts' expectations, and 3 in all other scenarios. The variables are defined the same as in the abnormal accrual model. We expect to observe the same signs on the coefficients of these variables in this model as in the abnormal accruals model.

IV. RESULTS

Descriptive Statistics

Table 2 presents descriptive statistics relating to the components of our composite measure of IAF quality and to the variables included in the model using abnormal accruals as the dependent variable (Panels A and B, respectively). Panel C presents Spearman and Pearson correlation data for the variables included in our model. The average experience in internal auditing for internal audit groups included in our sample is 6.35 years. The average percentage of internal auditors that have a professional certification (CIA or CPA) in our sample is 59 percent. Of the CAEs in our sample, 69 percent report directly to the audit committee. Internal auditors in our sample spend, on average, 32 percent of their time performing work that relates directly to financial reporting and 59 hours per year training. Finally, companies in our sample spend, on average, 51 percent more on the IAF than the industry average for all GAIN respondents.[21]

As shown in Panel C of Table 2, of the six correlation pairs among the four governance variables, three are significant. *ACEffectiveness* varies positively with *AuditorSpecialist*, and *Gindex* varies negatively with *AuditorSpecialist*. Internal audit quality is significantly correlated, but only modestly, with *ACEffectiveness*.[22]

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[21] The descriptive statistics in Panel A of Table 2 provide further evidence that we capture a broad range of IAF quality, as each individual characteristic varies significantly. This provides some assurance that our results may generalize to a larger population of firms despite potential issues relating to self-selection into the GAIN database.

[22] There are at least three possible reasons for the relative lack of significant correlations between our measures and other measures of corporate governance. First, companies may not systematically choose high levels of all components of corporate governance but may instead make different trade-offs among them. Note that the three other measures of corporate governance also show modest or no intercorrelation. Second, consistent with the standards of the IIA, most IAFs establish a degree of independence from management as a part of maintaining objectivity. Many of the specific decisions relating to the quality of an IAF (certification levels of new hires, training, etc.) typically are determined by the CAE, and, thus, it should not be particularly surprising that these variables may not move in concert with broader governance choices such as independence of audit committee members, quality of external auditor, etc. Third, the measures of corporate governance are likely formative—as opposed to reflective—indicators of a theoretical construct of overall corporate governance quality. For a discussion of the difference between formative and reflective indicators, see Jarvis et al. (2003) and Mackenzie et al. (2005).

Multivariate Results for Abnormal Accruals

Table 3 summarizes the results from regressing absolute and signed measures of abnormal accruals on internal audit quality.[23] In Panel A, we observe that *IAQuality* is significantly negatively related to *AbnAccr* (p-value < 0.10). This result is consistent with the idea that a higher-quality IAF is associated with lower levels of earnings management. Further examination of the results for abnormal accruals suggests that the moderating effect of internal audit quality is largely driven by the negative abnormal accrual firm-year responses. As shown in Panels C and B, respectively, *IAQuality* is significantly related to *−AbnAccr* (p-value < 0.05) but not to *+AbnAccr* (p-value > 0.10). These results are consistent with the notion that higher-quality IAFs help limit big-bath behavior or other potentially opportunistic behavior, such as reserve padding, on the part of management.

TABLE 3

Tests of Internal Audit Function Quality's Relation with Abnormal Accruals

Variable	Prediction for Absolute Value of Accruals	Panel A: Absolute Value of Accruals		Panel B: Positive Accruals		Panel C: Negative Accruals	
		β	z-value	β	z-value	β	z-value
Intercept	?	0.0919	2.16**	0.0252	0.76	−0.0987	−1.79*
IAQuality	−	−0.0039	−1.30*	0.0011	0.46	0.0083	2.02**
ACEffectiveness	−	−0.0006	−0.39	−0.0006	−0.49	0.0007	0.45
AuditorSpecialist	−	−0.0096	−1.12	−0.0124	−1.61*	0.0002	0.02
Gindex	+	0.0013	0.69	−0.0016	−1.27	−0.0018	−0.82
Assets	+	−0.0063	−1.34	0.0006	0.16	0.004	0.66
Age	−	−0.0008	−1.77**	−0.0001	−0.3	0.0013	2.51**
Leverage	+	0.0543	1.44*	0.0367	1.67**	−0.0563	−1.30*
Complexity	+	0.0017	0.92	0.0047	2.87***	−0.0012	−0.5
CFO	−	0.0000	0.53	−0.0000	−1.55*	−0.0000	−0.43
SalesGrowth	+	−0.0248	−1.42	0.0048	1.16	0.0523	1.66**
MB	−	−0.0005	−1.56	0.0004	3.11***	0.0006	3.83***
CFOVolatility	+	0.0000	1.17	0.0000	1.95**	−0.0000	−0.76
ROA	−	0.5034	2.69***	0.0227	0.49	−0.6164	−3.20***
Loss	+	0.0415	2.49***	−0.0099	−0.85	−0.0531	−2.62***
NYSE	?	0.0054	0.43	−0.0087	−0.76	−0.0113	−0.71
n		528		178		350	
Adj. R^2 (from OLS regression)		0.233		0.288		0.288	

*, **, *** Indicate statistical significance at the p-value ≤ 0.10, 0.05, and 0.01 levels, respectively. Cluster-adjusted standard errors are used to compute z-statistics. Industry and year effects are repressed for presentational ease. p-values represent one-tailed tests when a specific direction is predicted and the sign of the coefficient is consistent with that prediction.

The predicted signs for the absolute value of accruals (Panel A) also apply to positive accruals (Panel B), but are opposite for negative accruals (Panel C).

See the Appendix for variable descriptions.

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[23] We performed standard regression diagnostics. The variance inflation factor (VIF) scores were all below four, suggesting that multicollinearity was not a problem (Neter et al. 1990). The effect of outliers was measured using Cook's test, studentized residuals, dfFITS, and residual plots; no individual observations exerted undue influence on the results.

The lack of an association between the IAF and positive abnormal accruals might be attributable to a difference in emphasis between internal and external auditors. Research by Francis and Krishnan (1999) provides evidence that external auditors are more conservative with respect to income-increasing than income-decreasing accruals. Kim et al. (2003) find that higher-quality external auditors (i.e., Big 6 firms) are more effective in deterring/monitoring opportunistic behavior than non-Big 6 external auditors in firms with income-increasing accruals, but that Big 6 external auditors are less effective than non-Big 6 external auditors at deterring opportunistic behavior when firms have incentives toward income-decreasing accruals. Together, this research suggests that the effect of the external auditors on mitigating earnings management in our data set is likely to be greater with respect to income-increasing accruals than income-decreasing accruals. The external auditor's influence on income-increasing accruals may thus limit the effect of internal auditing on such accruals. Consistent with this idea, Panel B of Table 3 shows that *AuditorSpecialist* is significantly negatively related to $+AbnAccr$ (p-value < 0.10) but is not related to $-AbnAccr$ (p-value < 0.10), while *IAQuality*, as previously discussed, is negatively related to $-AbnAccr$ (p-value < 0.05).

The coefficients on two of the three proxies for the other aspects of corporate governance, *ACEffectiveness* and *Gindex*, are not statistically significant; however, as discussed, the results for *AuditorSpecialist* are moderately significant in the direction predicted (p-value < 0.10) for income-increasing accruals. Taken as a whole, these results suggest that in our sample, internal audit quality plays a more significant role in moderating earnings management than do the other aspects of corporate governance.

Of the control variables included in our model, the coefficients on *Age*, *Loss*, and *Leverage* are significant in the expected direction (p-values < 0.10) and the *ROA* coefficient is significant in the opposite direction of that predicted. The other control variables are not significant at the p-value < 0.10 level.

Results for Meet/Beat versus Miss Analysts' Forecast

As a secondary measure of earnings management, we test whether *IAQuality* is associated with the propensity to just meet or beat analysts' consensus forecast. The companies that are most likely to have managed earnings are those that meet/just beat analysts' expectations. Therefore, as previously explained, we compare these firms against each other and against all other observations.

Table 4 presents our results comparing the IAF quality of companies that just met or beat analysts' expectations to those that just missed analysts' expectations. We also compare these two groups to all other companies in the sample to identify differences across the entire sample. Panels A, B, and C of Table 4 present descriptive statistics for the three groupings of observations used in our multinomial logit model (i.e., just missed/just beat, just missed/all others, just beat/all others). Consistent with our expectations, IAF quality is highest in the sample of firm-years that just missed forecasts (*IAQuality* = 2.74) and lowest in the sample of firms that just met the forecast (*IAQuality* = 2.37).

Panels D, E, and F of Table 4 present the pairwise comparisons of the three groupings. In Panel D, we compare firms that just missed analysts' expectations to all other firms (i.e., those that met, beat, or missed by a large margin) in the sample. Panel E shows results from our comparison of firms that just met or beat analysts' forecasts with all other firms in the sample (i.e., those that just missed and those that beat or missed by a large margin). Panel F details the comparison of firms that just missed expectations to those that just met or beat expectations. The results suggest that companies that just missed analysts' forecasts have significantly higher-quality IAFs compared to all other companies ($\chi^2 = 2.75$, p-value

TABLE 4
Tests of Internal Audit Function Quality's Relation with Beating/Missing Analysts' Forecasts

	Descriptive Statistics			Pairwise Comparisons from Multinomial Logit Estimations					
	Panel A: Missed Forecast	Panel B: All Other Firms	Panel C: Just Made Forecast	Panel D: Missed Compared to All		Panel E: Just Made Compared to All		Panel F: Missed Compared to Just Made	
Variable	Mean (Std. Dev.)	Mean (Std. Dev.)	Mean (Std. Dev.)	β	χ^2	β	χ^2	β	χ^2
Intercept	n = 42	n = 390	n = 139	−5.9456	6.38**	−3.0706	5.89**	−2.8750	1.34
IAQuality	2.74 (1.13)	2.41 (1.16)	2.37 (1.07)	0.2928	2.75**	0.0195	0.03	0.2733	2.05*
ACEffectiveness	8.67 (3.26)	9.44 (2.01)	9.43 (1.93)	−0.0836	1.39	0.0632	1.20	−0.1468	3.30*
AuditorSpecialist	0.64 (0.48)	0.49 (0.5)	0.46 (0.5)	0.5281	1.94*	0.0555	0.06	0.4726	1.37
Gindex	9.80 (2.18)	9.76 (2.46)	9.78 (2.2)	0.0123	0.03	0.00767	0.03	0.0046	0.00
Assets	9.44 (1.18)	9.20 (1.4)	9.19 (1.34)	0.1782	0.76	0.0843	0.52	0.0939	0.19
Age	48.29 (11.38)	43.09 (13.99)	42.05 (14.77)	0.0125	0.48	−0.0023	0.07	0.0148	0.63
Leverage	0.48 (0.13)	0.45 (0.17)	0.46 (0.17)	2.0766	2.37*	1.466	4.24**	0.6107	0.19
Complexity	4.71 (2.17)	3.37 (2.06)	3.6 (2.17)	0.2811	11.89***	0.0792	2.30	0.2018	5.27**
CFO	2,427 (4,228)	1,838 (3,886)	2,792 (4,408)	0.0000	0.38	0.00014	7.07***	−0.0001	2.15*
SalesGrowth	0.15 (0.28)	0.10 (0.37)	0.09 (0.15)	−0.0234	0.00	−0.2931	0.36	0.2697	0.20
MB	3.82 (5.68)	2.70 (11.9)	2.14 (25.31)	0.0066	0.17	0.000803	0.02	0.0058	0.13
CFOVolatility	570.43 (852.73)	581.62 (1,253.19)	533.85 (815.06)	−0.0001	0.04	−0.00059	5.50**	0.0005	2.73**
ROA	0.05 (0.05)	0.03 (0.06)	0.07 (0.08)	3.4715	0.64	5.8289	6.62***	−2.3574	0.28
Loss	0.05 (0.22)	0.17 (0.38)	0.07 (0.26)	−1.1968	2.03	−0.4774	1.26	−0.7194	0.64
NYSE	0.95 (0.22)	0.93 (0.26)	0.9 (0.3)	−0.5515	0.43	−0.2848	0.46	−0.2667	0.09

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*, **, *** Indicate statistical significance at the p-value ≤ 0.10, 0.05, and 0.01 levels, respectively.

Firm-year responses are categorized as "Missed Forecast" if they missed analysts' consensus forecast scaled by price at the beginning of the year by less than 0.0005, they are categorized as "Just Made Forecast" if they met the forecast or exceeded it by the consensus forecast scaled by price at the beginning of the year by more than 0.0005. All other firm-years are categorized as "All Other Firms."

p-values represent one-tailed tests when a specific direction is predicted and the sign of the coefficient is consistent with that prediction. Industry and year effects are repressed for clarity of presentation.

See Appendix for variable descriptions.

< 0.05) and compared to companies that just met/beat analysts' forecasts ($\chi^2 = 2.05$, p-value < 0.10). However, firms that just met/beat analysts' forecasts are not statistically different from all other firms ($\chi^2 = 0.03$). Taken as a whole, these results provide evidence that firms with higher-quality IAFs are more likely to just miss earnings benchmark targets, which is consistent with our expectation that high-quality internal audit functions reduce earnings management.

AuditorSpecialist is significantly higher for companies that missed forecasts compared to all other companies ($\chi^2 = 1.94$, p-value < 0.10). There are also differences between firm-year responses in terms of *Leverage*, *Complexity*, *CFO*, *CFOVolatility*, and *ROA*. Interestingly, *ACEffectiveness* is significantly lower in companies that missed forecasts compared to companies that just met their forecasts ($\chi^2 = 3.30$, p-value < 0.10). We do not have an explanation for this unexpected result, which might be fruitfully investigated in more depth by future research.

Sensitivity Analyses

In this section we describe two additional analyses. The first analysis addresses potential endogeneity concerns and the second provides information relating to the components used to construct *IAQuality*. Endogeneity can be a problem for three different reasons: correlated omitted variables, measurement error, or simultaneity. We believe the possibility of correlated omitted variables is the most serious of these potential concerns, and, thus, include measures for the corporate governance mechanisms other than the IAF (i.e., *ACEffectiveness*, *AuditorSpecialist*, and *Gindex*) in our primary analyses. Inclusion of controls for the effects of the audit committee, the external auditor, and management decreases the likelihood that the results for internal audit quality are attributable to correlated omitted variables relating to other aspects of corporate governance.

As previously discussed, our measure of overall internal audit quality is derived from specific quality components indicated by professional practice standards and is arguably a more precise measure of internal audit quality than those used in prior studies (see Prawitt et al. 2009). We do not believe measurement error is of particular concern in this study.

We also do not believe that simultaneity represents a serious threat. We acknowledge that in most organizations the audit committee sets the budget for the IAF; therefore, the audit committee (or management) could clearly influence internal audit quality via resource allocation decisions. However, choices concerning how the IAF invests its resources are typically made within the IAF by the CAE, and, as such, these more detailed decisions are not necessarily made simultaneously with overall decisions relating to corporate governance. Further, if internal audit quality were decided simultaneously with other aspects of corporate governance, we would expect measures of audit committee quality, external audit quality, and management power to vary systematically with IAF quality. Our results are not consistent with this explanation (see Table 2, Panel C).

However, we attempt to further mitigate the possibility of threats to validity relating to potential endogeneity concerns using a two-stage least-squares approach.[24] In the first stage, we predict internal audit quality using two instrumental variables. These variables are the average *IAQuality* score by industry[25] and the amount of inventory relative to assets. We also include *Assets*, *Leverage*, and *Complexity* in predicting internal audit quality, given the findings of Carcello et al. (2005) that these variables predict the size of the internal audit budget.[26] Table 5, Panel B presents the results of this test for the accruals models. When we estimate the two-stage least-squares model, we find that *IAQuality* is significant in the absolute abnormal accruals model in the predicted direction (p-value < 0.01; see Panel B, Regression 1). Of note, once we control for endogeneity using our two-stage least-squares model, *IAQuality* is both significantly positively associated with negative accruals (p-value < 0.01) and negatively associated with positive accruals (p-value < 0.10—see Regressions 2 and 3).

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[24] Identifying appropriate instruments is difficult in conducting a two-stage least squares analysis and finding incorrect instruments can lead to IV estimates that are more biased than simple OLS estimates. We encourage readers to review Larcker et al. (2008) for an excellent discussion of the potential problems and limitations associated with using an instrumental variables approach.

Panel C of Table 5 shows the results of the miss/beat analysts' expectations model incorporating the two-stage least-squares analysis. In this analysis, the coefficient on *IAQuality* is not significant. Thus, to the extent the two-stage least-squares procedure is successful in controlling for effects of endogeneity in this analysis, the primary results relating to our secondary dependent variable should be interpreted with caution. However, given the limited size of the data set available to us, it should be noted that the lack of significance for these results may be attributable to smaller sample sizes in the various "bins" and the relatively low power of two-stage least square designs (see Larcker et al. 2008).

Our second supplementary analysis explores the effects of the individual components of *IAQuality*. When we rerun the results for Tables 3 and Table 4 using the individual components of internal audit quality in place of our composite *IAQuality* measure, we find that *Certification* is the only component variable significant in the sample of firms that have negative abnormal accruals and that *Certification* is associated with a moderation of negative abnormal accruals (p-value < 0.01). In the positive abnormal accrual sample, *IASize* is significantly positive (p-value < 0.01), suggesting that larger IAFs relative to their industry are associated with income-increasing accruals. This unexpected positive relationship may arise because IAF size can proxy for the overall demand the company has for auditing (see Anderson et al. 1993; Prawitt et al. 2009). If a company has a greater demand for auditing, then the company may be more difficult to monitor and, therefore, managers may be able to exercise greater discretion and manage earnings to a greater extent.

In the benchmark tests of Table 4 relating to the meet/beat analyses, we find, as expected, that the group that just missed analysts' expectations had significantly higher values of *Certification*, *CAEAC*, and *Training* (p-value < 0.10) and significantly higher values of *TimeFin* than the "all other firms" sample. We also find that the group that just missed analysts' expectations had significantly higher values of *Certification*, *CAEAC*, *TimeFin*, and *Training* than firms that just met or beat the benchmark (p-values < 0.05).

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[25] Consistent with prior research (e.g., see Erickson et al. 2008), we look to the industry average of our primary firm-specific independent variable for use as an instrumental variable. The industry-wide average of *IAQuality* is likely to be associated with an individual firm's *IAQuality* because internal audit practices in one firm are likely influenced by industry-wide practices. In addition, the industry-wide *IAQuality* is unlikely to be correlated with earnings management because we do not find that earnings management is highly concentrated in a few industries. We use self-reported industries in the sample so as to have sufficient sample sizes in each industry to make a meaningful first-stage prediction. Two industry categories had seven and nine firm-year responses, but the rest included at least 24 observations per industry grouping.

[26] The results are qualitatively similar if we assume that *ACEffectiveness*, *AuditorSpecialist*, and *Gindex* are exogenous, and we include those variables as predictors of *IAQuality* in the first stage model.

TABLE 5
Two-Stage Least Squares Test of Internal Audit Function Quality's Relation with Abnormal Accruals and Meet/Beat versus Miss Analysts' Forecasts

Panel A: First Stage Model to Explain Internal Audit Quality

	Intercept	*IndIAQuality*	*InvAssets*	*Assets*	*Leverage*	*Complexity*	*NYSE*
Prediction	+/−	+	+	+	+	+	+
Coefficient	0.135	1.005	1.172	−0.129	0.466	−0.005	0.444
t-value	0.29	9.07***	1.88**	−4.02***	1.73**	−0.27	2.80***

Panel B: Second Stage Model to Test Relation with Abnormal Accruals

Variable	Prediction for Absolute Value of Accruals	Regression 1 Absolute Value of Accruals		Regression 2 Positive Accruals		Regression 3 Negative Accruals	
		β	z-value	β	z-value	β	z-value
Intercept	?	0.2051	4.12***	0.0659	1.54	−0.2565	−3.53***
IAQualityPrediction	−	−0.0436	−4.31***	−0.0137	−1.42*	0.0608	4.10***
ACEffectiveness	−	−0.0008	−0.56	−0.0007	−0.46	0.0005	0.31
AuditorSpecialist	−	−0.0051	−0.58	−0.0127	−1.61*	−0.0051	−0.41
Gindex	+	0.0005	0.29	−0.0016	−1.18	−0.0004	−0.2
Assets	+	−0.0135	−2.80***	−0.002	−0.53	0.0154	2.22**
Age	−	−0.0005	−1.17	−0.0000	−0.14	0.0008	1.39
Leverage	+	0.0779	2.03**	0.0403	1.70**	−0.0976	−2.19**
Complexity	+	0.0029	1.70**	0.0051	2.82***	−0.0028	−1.23
CFO	−	0.0000	0.28	−0.0000	−1.51	−0.0000	−0.28
SalesGrowth	+	−0.0214	−1.28	0.0054	1.22	0.0449	1.49
MB	−	−0.0005	−1.88*	0.0003	2.97***	0.0007	3.86***
CFOVolatility	+	0.0000	1.14	0.0000	1.83**	−0.0000	−0.75
ROA	−	0.4705	2.43***	0.0088	0.17	−0.5692	−2.80***
Loss	+	0.0334	1.92**	−0.0137	−0.96	−0.043	−2.08**
NYSE	?	0.0204	1.64	−0.0018	−0.14	−0.0259	−1.64

Panel C: Second Stage Multinomial Logit Model to Test Relation with Missing/Beating Analysts' Forecasts

Variable	Regression 1 Missed Compared to All		Regression 2 Just Made Compared to All		Regression 3 Missed Compared to Just Made	
	β	χ^2	β	χ^2	β	χ^2
Intercept	−6.0174	4.77**	−2.7831	3.44*	−3.2343	1.23
IAQualityPrediction	0.3053	0.33	−0.0910	0.09	0.3964	0.49
ACEffectiveness	−0.0844	1.46	0.0649	1.25	−0.1493	3.46*
AuditorSpecialist	0.5533	2.14*	0.0591	0.07	0.4942	1.50
Gindex	0.0133	0.03	0.0047	0.01	0.0085	0.01
Assets	0.1884	0.75	0.0693	0.32	0.1191	0.26
Age	0.0141	0.60	−0.0021	0.06	0.0162	0.74

(*continued on next page*)

TABLE 5 (continued)

Variable	Regression 1 Missed Compared to All		Regression 2 Just Made Compared to All		Regression 3 Missed Compared to Just Made	
	β	χ^2	β	χ^2	β	χ^2
Leverage	1.8282	1.83	1.4907	4.36**	0.3375	0.06
Complexity	0.2731	11.20***	0.0822	2.41	0.1909	4.67**
CFO	0.0000	0.34	0.0001	6.57**	−0.0001	1.99
SalesGrowth	0.0303	0.01	−0.2741	0.31	0.3043	0.25
MB	0.0070	0.18	0.0006	0.01	0.0063	0.15
CFOVolatility	−0.0001	0.06	−0.0006	5.53**	0.0005	2.46
ROA	3.8212	0.78	5.8034	6.54**	−1.9823	0.20
Loss	−1.0999	1.70	−0.4809	1.29	−0.6190	0.47
NYSE	−0.5533	0.40	−0.2372	0.30	−0.3161	0.13

*, **, *** Indicate statistical significance at the p-value ≤ 0.10, 0.05, and 0.01 levels, respectively.

Panel A is estimated for 571 firm-year responses (the meet/miss forecast sample). Cluster-adjusted standard errors are used to compute t-statistics in Panel B. In Panel C, firm-year responses are categorized as "Missed Forecast" if they missed analysts' consensus forecast scaled by price at the beginning of the year by less than 0.0005. They are categorized as "Just Made Forecast" if they met the forecast or exceeded it by the consensus forecast scaled by price at the beginning of the year by more than 0.0005. All other firm-years are categorized as "All Other Firms." Industry and year effects are repressed for presentational ease.

p-values represent one-tailed tests when a specific direction is predicted and the sign of the coefficient is consistent with that prediction.

See the Appendix for variable descriptions.

V. CONCLUSIONS

This paper investigates the effects of IAF quality on earnings management. Using a unique archive of survey responses collected and maintained by the IIA for benchmarking purposes, we test our expectation that IAF quality is associated with reduced levels of earnings management. We derive our measure for overall internal audit quality from auditing standards that provide guidance to external auditors in assessing the extent to which they can rely on the work of the IAF as part of the financial statement audit (AICPA 1997; PCAOB 2007a). We use absolute abnormal accruals from the performance-adjusted modified Jones model and propensity to just meet or beat, versus just miss, analysts' earnings forecasts as proxies for earnings management.

On balance, the results are consistent with the notion that IAF quality is negatively associated with earnings management. In particular, companies with higher-quality IAFs are associated with smaller negative abnormal accruals and are more likely to just miss analysts' earnings forecasts. From a broader perspective, these results suggest that IAFs play an important role in the quality of externally reported financial numbers, an essentially untouched but potentially promising area for future research.

The results of this study should prove interesting to several parties. Companies and their audit committees should consider the quality of their IAF as defined by external auditing standards and explore ways of improving the function in light of these findings. Internal audit standard-setters could consider these findings in their deliberations regarding the potential role of internal auditing in affecting financial numbers that are reported externally. The SEC, managements of stock exchanges, and other groups interested in corporate governance processes may also find it helpful to more explicitly consider the role of internal auditing in financial reporting. Finally, auditing standard-setters may find it informative that our results are supportive of the validity of components of internal audit quality suggested by external auditing standards.

This study has several limitations. First, since the sample was not randomly drawn, the extent to which we can be confident that our results generalize to other companies is somewhat limited. However, it should be noted that this is first time archival data about IAFs have been available at this level of detail. Further, descriptive statistics suggest the data set captures a reasonably broad range of IAF quality. While we believe any self-selection bias would be likely to work against finding evidence for our expectations, future studies could test the relations we identify in a broader sample, if such data becomes available. Second, due to limitations on data availability, our sample is relatively small compared to many archival studies examining earnings management. Future studies could use larger sample sizes when and if more data becomes available. Third, the data archived in the IIA's GAIN database is collected through a continuing survey. Therefore, despite the fact that response errors are likely to contribute noise rather than bias, the reliability of the relations we identify is contingent on the accuracy of the GAIN data. Fourth, the choice a company makes with regard to the quality of its IAF may be endogenous with other corporate governance decisions. We attempt to mitigate this problem by incorporating proxies for other corporate governance variables into our primary analysis and by using alternative econometric approaches. However, current econometric tools are limited in this regard and our results should be interpreted with appropriate caution. Finally, our measures of earnings management may not adequately capture the underlying construct. While we do find generally consistent results for two independent proxies for earnings management, future studies may wish to examine whether other proxies for aggressive financial reporting provide additional insight. For example, at some point sufficient data may become available to examine whether companies with high-quality IAFs are less likely to experience financial statement restatements, face SEC action, or to be sued for financial reporting irregularities.

APPENDIX
DESCRIPTION OF VARIABLES

Variable		**Description**
A_{it-1}	=	Average total assets (Data6) for estimation portfolio firm i for year $t-1$.
AbnAccr	=	Abnormal accruals is the error term as measured using the equation (absolute value is used in testing): $\frac{TA_{it}}{A_{it-1}} = \beta_0 + \beta_1\left(\frac{1}{A_{it-1}}\right) + \beta_2\left(\frac{\Delta REV_{it} - \Delta AR_{it}}{A_{it-1}}\right) + \beta_3\left(\frac{PPE_{it}}{A_{it-1}}\right) + \beta_4\left(\frac{NI_{it}}{A_{it}}\right) + e_{it}.$
ACEffectiveness	=	A composite score of ten dummy variables formed by scoring a company one point for each Blue-Ribbon Committee for Audit Committee Effectiveness attribute the company's audit committee possesses.
Age	=	The number of years the company is listed on Compustat.
Assets	=	Total assets (Data6) of a company (natural log used in testing).

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Variable		**Description**
AuditorSpecialist	=	A dichotomous variable that indicates whether the external auditor is an industry-specialist auditor or not. We define industry-specialist auditor as a Big N audit firm that provides within-industry market shares 30 percent greater than if the audit firms were to split the industry evenly among themselves.
CAEAC	=	A dichotomous variable indicating whether the CAE reports to the audit committee (= 1) or to a lower-level entity within the organization (= 0).
Certification	=	The percent of internal auditors who have the CIA or CPA certification. (Number of auditors with the CIA or CPA certification/Total staff in the IAF).
CFO	=	A companies' cash flows from operations (Data308).
CFOVolatility	=	The standard deviation of the companies' CFO for the previous five years.
Complexity	=	The number of operating segments that the company has.
e_{it}	=	Error term, our measure of abnormal accruals—labeled *AbnAccr*.
Experience	=	Average number of years of internal audit experience.
Gindex	=	Corporate governance metric computed by Gompers et al. (2003). For years the metric was not computed (i.e., 2001, 2003, and 2005), the average of the metric for the year before and after the missing year was used.
IAQuality	=	A single composite number measuring the quality of the IAF. The variable can range from 0 to 6 with 0 representing lowest quality and 6 representing highest quality. The score is formed by assigning a value of 1 to scores above the median for *Experience*, *Certification*, *CAEAC*, *TimeFin*, *Training*, and *IASize*, and summing together.
IAQualityPrediction	=	The predicted value of internal audit quality from regression in Table 5, Panel A.
IASize	=	The average dollar amount spent on internal auditing for the industry divided by the average total assets of the industry, subtracted from the dollar amount spent on internal auditing per company divided by the company's total assets. This amount is then divided by the average dollar amount spent on internal auditing for the industry, divided by the average total assets of the industry.
IndIAQuality	=	The average of *IAQuality* at the two-digit SIC code level.
IndustryDummies	=	Dichotomous variables used to represent different industries at the one digit SIC code level.
InvAssets	=	Inventory (Data3) divided by total assets (Data6).
Leverage	=	The sum of long-term debt (Data9) and current liabilities (Data5) for each company divided by total assets (Data6).
Loss	=	A dichotomous variable indicating whether the company experienced a loss in the previous year (yes = 1, no = 0).
MB	=	A company's market-to-book ratio (Data24 * Data25/Data216).
NI_{it}	=	Net income (Data172) for estimation portfolio firm *i* for year *t*.
NYSE	=	A dichotomous variable indicating whether the company is traded on the NYSE or not (yes = 1, no = 0).
PPE_{it}	=	Gross property, plant, and equipment (Data7) for estimation portfolio firm *i* for year *t*.

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Variable		Description
ROA	=	Return on assets (Data172/Data 6).
SalesGrowth	=	A company's one-year sales growth (Data12 − Data12 prior year/Data12 prior year).
TA_{it}	=	Total accruals for estimation portfolio firm *i* for year *t*. TA_{it} are defined as income before extraordinary items (DATA18) minus operating cash flows (DATA308).
TimeFin	=	The percentage of time the IAF spends performing financial audit work.
Training	=	The average number of training hours internal auditors attend during the year.
Year Dummies	=	Dichotomous variables used to represent different years of data included in the analysis.
ΔREV_{it}	=	Change in net revenues (Data12) for estimation portfolio firm *i* for year *t*.
ΔAR_{it}	=	Change in accounts receivable (Data2) for estimation portfolio firm *i* for year *t*.

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The Voluntary Adoption of Internationally Recognized Accounting Standards and Firm Internal Performance Evaluation

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ABSTRACT: A large body of research is devoted to understanding the causes and consequences of firms' adoption of internationally recognized accounting standards. Thus far, researchers' attention has focused almost exclusively on the informational benefits of the adoption. We extend the existing literature by offering a different, stewardship perspective. We hypothesize that the voluntary adoption of international accounting standards is associated with changes in the firm's internal performance evaluation process; in particular, it is associated with increases in the sensitivities of CEO turnover and employee layoffs to accounting earnings. Our results are consistent with these predictions.

Keywords: *international accounting standards; performance evaluation; CEO turnover; layoffs.*

Data Availability: *All data are available from commercial providers (Worldscope, SDC, Spectrum, and I/B/E/S).*

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I. INTRODUCTION

There has been a significant recent movement around the world toward an internationally recognized set of accounting standards.[1] A large body of academic research is devoted to this phenomenon, examining both the motivation for adopting international accounting standards and its economic consequences (e.g., Ashbaugh 2001; Leuz and Verrecchia 2000; Ashbaugh and Pincus 2001; Lang et al. 2003; Bradshaw and Miller 2004; Barth et al. 2008; Daske et al. 2008). This research has focused almost exclusively on the *informational* benefits of the adoption; whereas it has long been recognized that *stewardship* demands also substantially shape firms' accounting choices (e.g., Watts and Zimmerman 1986; Ball 2001; O'Connell 2007). We broaden the scope of the current literature by investigating the adoption of international accounting standards from a stewardship perspective in terms of firm internal performance evaluations. The case of Daimler-Benz AG offers an illustrative anecdote. Daimler-Benz summarized in its 1996 financial statements that its U.S. GAAP adoption (after U.S. cross-listing in 1993) not only affected its "external reporting," but the same U.S. GAAP numbers were also used for "internal controlling" purposes to increase the transparency and efficiency of "internal reporting" and improve the performance measurement of the company and its business units (see Ball [2001, 2004] for more detail).

We analyze whether the voluntary adoption of internationally recognized accounting standards (i.e., IFRS or U.S. GAAP) by non-U.S. firms is associated with changes in the role accounting earnings play in firms' internal performance evaluations. Principal-agent theory suggests that the weight a performance measure receives in optimal compensation contracts increases with its informativeness about the agent's actions (Holmstrom 1979; Lambert and Larcker 1987; Bushman and Smith 2001). If IFRS/U.S. GAAP earnings are more informative about firm performance than local GAAP earnings, we expect the switch to IFRS/U.S. GAAP to be accompanied by a greater emphasis on accounting earnings in the internal performance evaluation process, implying higher sensitivities of CEO turnover to accounting earnings post-adoption.[2] Furthermore, if the more informative IFRS/U.S. GAAP earnings better enable firms to identify underperforming divisions, we expect employee layoffs to also be more sensitive to accounting earnings post-adoption.

Prior evidence indicates that voluntary IFRS/U.S. GAAP adoption comes with significant changes in firms' financial reporting properties, with earnings being more informative, less managed, and exhibiting more timely loss recognition (e.g., Barth et al. 2008; Lang et al. 2003). Voluntary IFRS/U.S. GAAP adoption is also associated with reduced cost of capital (Leuz and Verrecchia 2000) and lower analyst earnings forecast errors (Ashbaugh and Pincus 2001), suggesting that IFRS/U.S. GAAP earnings are more informative, less likely to be obfuscated by earnings management, and thus a better reflector of managerial effort and firm underlying performance than local GAAP earnings. Some argue that accounting conservatism contributes to optimal compensation contracting and firm governance (e.g., Ball et al. 2000; Ball 2001; Watts 2003a and 2003b; Leone et al. 2006; LaFond and Watts 2008). The attractiveness of IFRS/U.S. GAAP earnings for internal performance evaluation is further enhanced by their greater conservatism through more timely loss recognition than local GAAP earnings.

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[1] Both IFRS (International Financial Reporting Standards) and U.S. GAAP are commonly perceived as "internationally recognized" accounting standards, although IFRS have gained greater momentum recently. According to the IASB, close to 100 jurisdictions have now mandated IFRS financial reporting, including the adoption of IFRS by all European Union listed firms since 2005 (http://www.iasplus.com/country/useias.htm). In addition, many firms adopted IFRS/U.S. GAAP on a voluntary basis prior to their country's mandatory adoption. The international accounting standards (IAS) were renamed to IFRS in 2001; we use IFRS and IAS interchangeably.

[2] Due to the lack of data availability on top executive compensation for international firms, we focus on CEO turnover instead.

Our sample consists of firms from Continental Europe that voluntarily adopted IFRS or U.S. GAAP from 1988 to 2004.[3] Consistent with our predictions, we find that CEO turnover and employee layoffs are more sensitive to accounting earnings after IFRS/U.S. GAAP adoption. These findings support our hypothesis that accounting earnings play a greater role in firm internal performance evaluations after the adoption of international accounting standards. In addition, we investigate firms' decisions to adopt international accounting standards and proxy for the performance evaluation demand with two variables: closely held shares (Bushman and Piotroski 2006) and labor productivity. After controlling for various other factors, we find that greater performance evaluation demand (less closely held shares and lower labor productivity) is associated with a higher likelihood of IFRS/U.S. GAAP adoption.

The above evidence does not necessarily imply that the voluntary adoption of international accounting standards *causes* the changes in internal performance evaluations in terms of higher earnings performance sensitivities. Firms that voluntarily adopt IFRS/U.S. GAAP likely experience fundamental changes in their operations, financing, and corporate governance, and the adoption of international accounting standards can simply be an instrument for these profound changes.[4] Our findings suggest that the greater reporting transparency through international accounting standards likely plays a role (which may not be strictly causal, but is important nonetheless) in improving firms' internal performance evaluations. It is important to document that IFRS/U.S. GAAP adoption is associated not only with changes in firms' operating and information environment, as shown in prior studies, but also with changes in corporate governance.

We make several contributions to the literature. First, our study distinguishes from and complements the large body of academic work that focuses almost exclusively on the informational benefits of IFRS/U.S. GAAP adoption. We support the long-held notion that stewardship demands substantially shape firms' accounting choices (e.g., Watts and Zimmerman 1986; Ball 2001; Watts 2003a; Bushman et al. 2006) and provide an application of the contracting-related perspective to the fast-growing area of international accounting research. Our findings highlight the multitude of implications from the adoption of international accounting standards and add to our understanding of the complex changes experienced by adopting firms. Second, in light of the recent decision by the IASB/FASB to deemphasize stewardship as a primary objective of financial reporting, O'Connell (2007) calls for a renewed focus on stewardship-related research. Our study takes a step in that direction by highlighting the role of accounting information in corporate governance and internal performance evaluations.

We develop our hypotheses in Section II and discuss our methodology in Section III. Sections IV–VI present our main results. Robustness tests are in Section VII. Section VIII concludes.

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[3] Continental Europe accounts for a substantial portion of the voluntary IFRS/U.S. GAAP adoptions during our sample period. For example, out of the voluntary IFRS adopters around the world reported in Barth et al. (2008), about 60 percent come from Continental Europe (Chinese firms are the next largest group, accounting for 22 percent of the observations). We also focus on Continental Europe because of the contrast between the local GAAPs that permit or even encourage the creation and release of "hidden reserves," with IFRS/U.S. GAAP, which do not allow such practices (discussed in more detail later).

[4] It is also possible that an increase in the level of external monitoring (e.g., by institutional investors or regulators) may prompt a firm to simultaneously improve corporate governance (through heightened performance sensitivities) and increase corporate transparency (by adopting IFRS).

II. HYPOTHESIS DEVELOPMENT AND LITERATURE REVIEW

We build on evidence that accounting earnings are timelier and more conservative after voluntary IFRS/U.S. GAAP adoption and formulate our predictions regarding the sensitivities of executive turnover and employee layoffs to accounting earnings around the time of the adoption. We then discuss the likely reasons behind the changes in accounting earnings properties at the adopting firms.

Main Predictions

Both accounting earnings and stock returns affect management compensation as well as executive turnover (see Bushman and Smith [2001] for a review of the U.S. and international evidence). We argue that after the voluntary adoption of international accounting standards, accounting earnings assume a greater role in firm internal performance evaluations. In particular, we predict that the sensitivity of executive turnover to accounting earnings increases post-adoption. This prediction can be supported by two arguments.

First, prior research suggests that compared to local GAAP earnings, IFRS/U.S. GAAP earnings show greater timeliness and less evidence of earnings management (e.g., Barth et al. 2008; Lang et al. 2003), suggesting that they are better reflectors of managerial effort and firm underlying performance. As Watts (2003a) points out, greater timeliness in accounting earnings makes earnings a more effective contracting tool because earnings reflect managers' actions more precisely in each accounting period and this helps avoid the undesirable incentive outcomes due to managers' limited horizons. Principal-agent theory predicts that as the informativeness of a performance measure increases, its weight in the optimal compensation contract rises as a result (Holmstrom 1979; Lambert and Larcker 1987; Bushman and Smith 2001). This suggests that firms that have voluntarily adopted international accounting standards, which are associated with timelier and less managed earnings, likely place greater emphasis on their accounting earnings post-adoption for evaluating managers.

Second, compared to local GAAP earnings, IFRS/U.S. GAAP earnings also exhibit more conservative characteristics (e.g., Barth et al. 2008; Lang et al. 2003; Hung and Subramanyam 2007).[5] Some argue that accounting conservatism is part of the efficient contracting technology between a firm and its various stakeholders, including managers. Watts (2003a) cites the demands from executive compensation contracting and firm governance as important drivers of accounting conservatism. This is because managers' limited tenure and limited liability make it difficult for firms to recover overpayment to managers, therefore creating the need for asymmetric verifiability and conservatively measured accounting earnings. The evidence in Leone et al. (2006) is consistent with timelier loss recognition in accounting earnings mitigating the *ex post* settling up problem in management cash compensation. Accounting conservatism can be a particularly powerful tool in disciplining poorly performing managers because it speeds up the recognition of losses and provides the board and shareholders a reason to investigate the losses, and if necessary, to dismiss the manager (Watts 2003a). This mechanism helps stem the losses from negative present value projects; moreover, it deters managers from taking on such projects in the first place (Ball 2001). As a result, accounting conservatism likely plays an important role in firm governance and performance evaluation. The above arguments lead to the following prediction:

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[5] Using the methodology in Barth et al. (2008) and Lang et al. (2003), we confirm that earnings in our sample firms are significantly more conservative (there is a greater frequency of large losses) post IFRS/U.S. GAAP adoption.

H1: The sensitivity of CEO turnover to accounting earnings increases after a firm adopts IFRS/U.S. GAAP.

We next turn to the wider workforce. Since IFRS/U.S. GAAP earnings exhibit greater timeliness and more conservatism, they likely better enable firms to identify underperforming divisions and subsequently engage in the necessary restructuring activities. As Liberty and Zimmerman (1986) point out, even though accounting earnings may not enter directly into labor contracts, as in the cases of management compensation contracts or debt contracts, accounting numbers can nevertheless affect the employment outcome of the labor force. The evidence in DeAngelo and DeAngelo (1991) and D'Souza et al. (2001) supports this notion. We collectively measure the career outcomes of a firm's employees through layoffs (defined as a reduction of a firm's employee headcount of 5 percent or more in a particular year) and investigate its sensitivity to the firm's accounting performance and expect employee layoffs to be more sensitive to accounting earnings post-adoption.

H2: The sensitivity of employee layoffs to accounting earnings increases after a firm adopts IFRS/U.S. GAAP.

What Drives the Changes in Earnings Properties after IFRS/U.S. GAAP Adoption?

Voluntary adopters of the international accounting standards experience significant changes in accounting earnings properties with earnings becoming timelier, less smoothed, and more conservative after the adoption (e.g., Barth et al. 2008; Lang et al. 2003; Hung and Subramanyam 2007). These findings, however, do not imply that the documented changes in accounting properties are *caused* by the changes in accounting standards per se. Voluntary adoptions are inevitably associated with firm-level incentives, for example, to access the capital markets, to cross-list in another country, to enter into foreign product markets, or as implied by our study, to improve performance evaluation and corporate governance. All of these provide genuine incentives for the adopting firms to improve accounting transparency. A recent study by Christensen et al. (2007) illustrates the importance of preparers' incentives by contrasting a sample of voluntary IFRS adopters in Germany with German firms that switched to IFRS following the EU mandate in 2005. They find less earnings management and more timely loss recognition after the *voluntary* adoptions; while no such evidence is documented for the *mandatory* adopters.

Given the firm-level incentives associated with voluntary adoptions of IFRS/U.S. GAAP, certain provisions in IFRS and U.S. GAAP also facilitate firms' efforts to provide more informative financial statements. A major vehicle for income smoothing and earnings management by Continental European firms is the use of "hidden reserves," the essence of which is to make excessive provisions for potential future losses.[6] Companies in Continental Europe are expressly allowed by law to set up reserves for *unspecified* potential future losses under the "prudence" concept and later draw upon the reserves to cover poor performance (Ball 2004; Alexander and Archer 2001). Although it is hard to assess the extent of hidden reserves in reported financial statements, such practices are believed to be widespread in Continental Europe.[7]

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[6] To create a reserve, a company makes excessive provisions for a potential future loss by increasing an expense account. The opposite side of the accounting entry can be either a reduction of an asset account, an increase of a liability account, or an increase to an equity reserve account (Ball 2004). The above entry can be reversed when needed later to release the reserve and increase income.

On the other hand, hidden reserves are prohibited under both U.S. GAAP and IFRS, significantly reducing the latitude of earnings management at the adopting firms. FAS No. 5, *Accounting for Contingencies* (effective since 1975; FASB 1975), lays out stricter conditions for recording loss contingencies. In paragraph 8, it states that "[a]n estimated loss from a loss contingency shall be accrued by a charge to income if both of the following conditions are met: a. ... it is probable that an asset had been impaired or a liability had been incurred ... b. the amount of loss can be reasonably estimated." The standard also specifically prohibits the provision of "general" contingencies by stating that "[s]ome enterprises have in the past accrued so-called 'reserves for general contingencies.' General or unspecified business risk does not meet the conditions for accrual in paragraph 8, and no accrual for loss shall be made." Similar provisions prohibiting the recognition of unspecified loss contingencies are found in IAS 10 (effective from 1978).[8]

The above arguments are consistent with the broad perception that international accounting standards are associated with greater accounting transparency and more informative reporting. For example, the following is an excerpt from a Reuters report in 1995 on "Deutsche to Shift to IAS Accounting Standard":

> Deutsche Bank AG, Germany's largest commercial bank, said on Tuesday it would present its 1995 results in accordance with International Accounting Standards, the first German bank to do so. "*With this step we will meet higher standards for improved transparency and information quality*," board member Juergen Krumnow was quoted as saying in a statement ... Deutsche joins chemical giant Bayer AG, pharmaceuticals leader Schering AG and Germany's largest industrial conglomerate Daimler-Benz AG in *providing greater insight into their books than more opaque German accounting methods offer. International Accounting Standards prohibit hidden reserves and demand more information on risk provisions—a key omission in German accounting systems.* (emphasis added)

In summary, the changes in accounting earnings properties experienced by firms that have voluntarily adopted IFRS/U.S. GAAP reflect these firms' incentives to improve their financial reporting transparency. This is facilitated by the IFRS/U.S. GAAP guidelines, including those against the use of hidden reserves. The resulting accounting earnings under international accounting standards exhibit less earnings management and are timelier in reflecting economic losses, making them more effective tools for internal performance evaluations and corporate governance.

Related Studies

The large and growing body of research related to international accounting standards can be classified into three areas: (1) studies of the motives for adopting IFRS/U.S. GAAP;

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[7] For example, the *European Accounting Guide* (Alexander and Archer 2001, page 1360) has the following discussion for Switzerland: "[T]he creation of hidden reserves is expressly allowed by law ... By definition, the extent of hidden reserves in financial statements is difficult to assess. An economic journal, however, in collaboration with a Swiss securities rating company, published from 1989 to 1992 a ranking of the most profitable Swiss companies, based on an estimation of their 'true' (undistorted) income. In some cases, reported income represents less than 25 percent of estimated real earnings. When confronted with the financial statements of Swiss companies, analysts must be conscious of the widespread use of hidden reserves."

[8] IAS 37, *Provisions, Contingent Liabilities and Contingent Assets* (IAS 1998), which became effective in 1999, replaced parts of IAS 10. However, IAS 37 does not change the essence of the accounting treatment for loss contingencies.

(2) analyses of the changes in accounting properties associated with the adoption; and (3) investigations of the economic consequences of the adoption (e.g., changes in the cost of capital). Below we summarize the literature along these lines.

Most studies of the voluntary adoptions of international accounting standards (IFRS or U.S. GAAP) explicitly model the adoption decision. The model specifications vary across studies; however, they include common elements that are associated with firm disclosure incentives such as firm size, performance, financing needs, and the need to communicate with foreign investors (e.g., Harris and Muller 1999; Leuz and Verrecchia 2000; Ashbaugh 2001).

Researchers also document that voluntary IFRS/U.S. GAAP adopters experience significant changes in accounting earnings properties. Barth et al. (2008) analyze a comprehensive sample of voluntary IFRS adopters around the world and document that relative to local GAAP earnings, IFRS earnings exhibit greater informativeness, less earnings management, and timelier loss recognition. A recent study by Hung and Subramanyam (2007) of a sample of voluntary IFRS adopters in Germany finds similar evidence, in that the IFRS earnings are less smoothed and more conservative than German GAAP earnings.[9] Similar inferences are drawn by Lang et al. (2003) for U.S. GAAP earnings in a sample of U.S. cross-listed firms. Christensen et al. (2007) contrast German firms that have voluntarily adopted IFRS with those that adopted IFRS under the EU mandate in 2005 and document less earnings management and timelier loss recognition in accounting earnings post-adoption *only* for the *voluntary* adopters.

Various studies examine the economic consequences of the adoption of international accounting standards. Leuz and Verrecchia (2000) report, for a sample of German firms, that the voluntary adoption of IFRS/U.S. GAAP is associated with a reduced information asymmetry component of the cost of capital. Bradshaw and Miller (2004) document that firms adopting U.S.-like accounting standards see increases in U.S. institutional holdings. Along a similar line, Covrig et al. (2007) analyze data on mutual fund holdings around the world and find that voluntary IFRS adopters have greater ability to attract foreign capital. In addition, Ashbaugh and Pincus (2001) document that voluntary IFRS adoption is associated with lower analyst earnings forecast errors. Finally, Daske et al. (2008) investigate the economic consequences of *mandatory* IFRS adoptions around the world and find evidence of reduced cost of capital after the adoption, although this evidence is concentrated in countries with stronger enforcement of accounting standards, again highlighting the importance of preparers' incentives in bringing about real changes in financial reporting.

III. METHODOLOGY

Our sample consists of Continental European companies that voluntarily adopted IFRS/U.S. GAAP between 1988 and 2004. We require the adopting firms to have both pre- and post-adoption data, and as a result, exclude firms that report under IFRS/U.S. GAAP from the first year they enter into our sample. Financial and price data are from the Worldscope database. We obtain information on debt and equity issuance, mergers and acquisitions, and divestitures (including sales of divisions, spin-offs, split-offs, and carve-outs) from the SDC database. U.S. institutional holdings data are from Spectrum and analyst-following data are from I/B/E/S. For the analysis of CEO turnover, we collect company officer names from the January edition of Worldscope CD-ROMs from 1992 to 2002. As discussed in DeFond and Hung (2004), top executive titles vary by country. We follow their procedure and collect the names of the officers under the titles of Chief Executive Officer, Chief Executive, CEO, or a country-specific title, and collectively refer to these top officers as the CEO.[10] When there are multiple individuals with CEO-like titles in a firm-year, we retain the name of the first individual reported by Worldscope. After identifying the CEO for each company in each year, we compare the CEO names between successive years to determine if there is a CEO turnover in a particular firm-year.

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[9] Unlike Barth et al. (2008), Hung and Subramanyam (2007) do not find IFRS earnings to be more value relevant than German GAAP earnings. As pointed out by Barth et al. (2008), the different findings are likely due to differences in methodology. Hung and Subramanyam (2007) have a fairly small and unique sample of German firms and make within firm-year comparisons of German GAAP and IFRS earnings immediately before the IFRS adoption, where the adopting firms may have made adjustments to their German GAAP statements in order to avoid large reconciliation items with IFRS. In addition, Tendeloo and Vanstraelen (2005) do not find differences in earnings management activities between German GAAP earnings and IFRS earnings. Again, this study is based on a short sample period (1999–2001) and a small sample of German firms.

We classify firms into those following IFRS/U.S. GAAP accounting standards and those following local accounting standards based on the information provided by Worldscope.[11] For the IFRS/U.S. GAAP adopting firms, the adoption year is treated as event year 0. The local standards firms (firms that follow local GAAP throughout our sample period) serve as the control sample in the various tests. We randomly select an event year 0 for these firms. Our sample includes firms that become cross-listed on major U.S. exchanges during our sample period. U.S. cross-listing automatically entails the reconciliation of net income and shareholders' equity with U.S. GAAP; however, these firms are classified as adopting firms only if their home country financial statements also follow U.S. GAAP or IFRS.[12,13]

To analyze firms' adoption decision, we require data on stock returns, accounting earnings, total assets, market capitalization, leverage, growth, foreign sales, and sales per employee one year prior to event year 0, and closely held shares for event year 0.[14] Our final sample comprises 200 IFRS/U.S. GAAP adopting firms and 766 local standards firms.[15] The vast majority of our sample of adopting firms (180 firms, or 90 percent of the adopting sample) follow IFRS and the rest choose U.S. GAAP.[16]

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[10] DeFond and Hung (2004, Table 1) provide country specific top executive titles, for example, "managing director" in Belgium, Denmark, Finland, and Italy; "president" in Sweden; and "chairman, board of management" in Austria, Germany, and The Netherlands. We exclude all titles involving the term "supervisory board" because it is distinct from the "management board" under the two-tier board structure observed in countries such as Austria, Germany, and The Netherlands.

[11] The coding is based on the information in Worldscope field 07536, *Accounting standards followed.* Following the procedure in Daske et al. (2007), if the Worldscope description is "IFRS, International standards," "International standards and some EEC guidelines," "Local standards with EEC and IASC guidelines," "Local standards with OECD and IASC guidelines," "Local standards with some IASC guidelines," then we classify the observation as using IFRS; if the Worldscope description is "U.S. standards (GAAP)" or "U.S. GAAP reclassified from local standards," then we classify the observation as using U.S. GAAP. Finally, all other cases, except when the applicable accounting standards are not disclosed, are classified as using local accounting standards. As the Worldscope terminology suggests, some of our sample IFRS/U.S. GAAP adopting firms may do so only partially, as the term "local standards" is mentioned alongside IFRS or U.S. GAAP. However, our predictions in Section II apply to these firms as well. Furthermore, out of our sample of 200 adopting firms, the vast majority, over 80 percent, have a straightforward Worldscope classification of following IFRS (46 firms), International Accounting Standards (98 firms), or U.S. Standards (GAAP; 19 firms).

[12] Arguably, firms that cross-list on major U.S. exchanges may use their reconciled U.S. GAAP earnings for internal performance evaluations even if their home country financial statements follow local GAAP. However, the influence of international accounting standards earnings is likely substantially greater if the firm's entire home country financial statements are prepared according to IFRS/U.S. GAAP. For firms with Level II or Level III ADRs in the U.S. and that adopt IFRS or U.S. GAAP, we set event year 0 to be the earlier of the adoption year or the first year of cross-listing.

[13] We exclude German firms listed on the New Market (1997–2003) from our analysis, as IFRS or U.S. GAAP adoption is part of the listing requirement of the German New Market. Most of these firms would not have satisfied our sample requirement in the first place because they follow IFRS/U.S. GAAP from the first year they enter the Worldscope database. Including the remaining (seven) New Market firms in our tests does not affect the inferences.

[14] *Close_Held* is measured in event year 0. Using the lagged year's measure results in more missing observations but does not affect the paper's inferences.

Table 1 reports our sample distribution by calendar year, country, and industry. Panel A presents the time-series distribution of our sample firms based on event year 0. The frequency of IFRS/U.S. GAAP adoption increases in more recent years, consistent with the trend of greater accounting harmonization around the globe. Panel B reports that Germany and Switzerland are heavily represented in the adopting sample; similar findings are also reported in Barth et al. (2008). Finally, Panel C suggests that manufacturing firms have the largest representation in the adopting sample, again similar to Barth et al. (2008) on IFRS adopters and Lang et al. (2006) on cross-listed firms in the U.S. In order to control for the country, year, and industry effects, all of our subsequent regression analyses include country, year, and industry dummies as control variables.

Table 2, Panel A presents the summary statistics around event year 0 for the variables in our IFRS/U.S. GAAP adoption decision analysis, separately for the adopting and local standards samples. All continuous variables are trimmed at the top and bottom 1 percent. The adopting sample has a significantly lower percentage of closely held shares. This is consistent with more widely held firms, facing greater information need from investors and also due to internal performance evaluation demands (as postulated in this study), are more likely to adopt international accounting standards. The adopting firms generally possess stronger disclosure incentives, as evidenced by their larger size, higher leverage, larger analyst following, and greater U.S. institutional holdings than local standards firms. Furthermore, the adopting firms tend to have greater capital needs than local standards firms, indicated by their higher frequencies of debt and equity issuance and acquisition activities. Finally, the adopting firms likely face greater information demand from foreign stakeholders, with higher frequency of U.K./U.S. cross-listings and greater percentage of foreign sales relative to total sales, than local standards firms.

Panel B of Table 2 reports the management turnover rates for our sample countries. Belgium, The Netherlands, Sweden, and Finland are dropped from this part of the analysis because each country has a low number of firm-year observations (fewer than ten) with non-missing management turnover data in either the pre- or the post-adoption period among the adopting firms. The remaining countries include Austria, Denmark, France, Germany, Italy, and Switzerland, which account for the 803 firm-year observations in the adopting sample and 1,722 firm-year observations in the local standards sample in our management turnover tests. The sample period for the management turnover tests is shorter than our overall sample period of 1988 to 2004 because non-missing CEO turnover data are available on Worldscope from 1992 to 2002. The turnover rates vary by country, although they match up reasonably well with those reported in DeFond and Hung (2004, Table 2). The overall turnover rate in the adopting sample (17.93 percent) is significantly higher than that in the local standards sample (12.37 percent).

In Table 2, Panel C we report by country the rates of employee layoffs (defined as a reduction of employee headcount of 5 percent or more in a firm-year). Switzerland is not included in this part of the analysis because of missing union data that are required later

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[15] We include all local standards firms in our analysis. An alternative approach is to create a matched sample of local standards firms based on criteria such as country, year, and industry. We choose to incorporate all local standards firms due to methodological concerns about the matched-pairs research design (e.g., Zmijewski 1984).

[16] Our results are robust if we include only IFRS adopters in our analysis.

TABLE 1
Frequency Distribution of the Sample

Panel A: Distribution of Event Year Zero by Calendar Year

	Adopting		Local Standards	
Year	**Frequency**	**Percent**	**Frequency**	**Percent**
1988	2	1.00	5	0.65
1989	6	3.00	13	1.70
1990	4	2.00	24	3.13
1991	6	3.00	42	5.48
1992	10	5.00	45	5.87
1993	9	4.50	38	4.96
1994	2	1.00	53	6.92
1995	5	2.50	52	6.79
1996	6	3.00	49	6.40
1997	8	4.00	53	6.92
1998	11	5.50	47	6.14
1999	30	15.00	50	6.53
2000	18	9.00	36	4.70
2001	15	7.50	36	4.70
2002	26	13.00	60	7.83
2003	18	9.00	68	8.88
2004	24	12.00	95	12.40
	200	100	766	100

Panel B: Distribution of Adopting and Local Accounting Standards Firms by Home Country

	Adopting		Local Standards	
Country	**Number of Firms**	**Percent**	**Number of Firms**	**Percent**
Austria	12	6.00	5	0.65
Belgium	8	4.00	33	4.31
Denmark	11	5.50	43	5.61
Finland	7	3.50	51	6.66
France	22	11.00	274	35.77
Germany	88	44.00	188	24.54
Italy	4	2.00	2	0.26
The Netherlands	1	0.50	73	9.53
Sweden	7	3.50	73	9.53
Switzerland	40	20.00	24	3.13
	200	100	766	100

Panel C: Distribution of Adopting and Local Accounting Standards Firms by Industry

	Adopting		Local Standards	
Industry	**Number of Firms**	**Percent**	**Number of Firms**	**Percent**
Mining	7	3.50	44	5.74
Construction	46	23.00	164	21.41

(*continued on next page*)

TABLE 1 (continued)

Industry	Adopting Number of Firms	Percent	Local Standards Number of Firms	Percent
Manufacturing	86	43.00	235	30.68
Utilities	21	10.50	33	4.31
Wholesale and Retail Trade	21	10.50	125	16.32
Finance, Insurance, and Real Estate	6	3.00	81	10.57
Services	13	6.50	84	10.97
	200	100	766	100

This table reports the distribution of the firms included in the adoption decision analysis by year (adoption year for adopting firms and a randomly selected year for non-adopting firms), home country, and industry. Adopting firms refer to firms that adopt the IFRS or U.S. GAAP between 1988 and 2004. Local Standards firms are those that report under local accounting standards over our entire sample period.

in the layoff regressions. The final sample in this part of the analysis includes 1,930 firm-year observations in the adopting sample and 8,228 firm-year observations in the local standards sample. The layoff rates vary by country and the overall layoff rate in the adopting sample (21.87 percent) is significantly lower than that in the local standards sample (25.12 percent).

IV. MODELING THE ADOPTION DECISION

We expand on the adoption decision models in the prior literature and test if the demand from internal performance evaluations is a factor in firms' decisions to adopt international accounting standards. We estimate the following logistic regression model (firm subscripts are suppressed):

$$\begin{aligned} Prob\,[Adopt = 1] = Logit(&a_0 + a_1 Close\_Held_0 + a_2 Labor\_Prod_{-1} \\ &+ a_3 RET_{-1} + a_4 ROA_{-1} + a_5 Size_{-1} + a_6 Lev_{-1} \\ &+ a_7 Growth_{-1} + a_8 Analyst_{-1} + a_9 Institution_{-1} \\ &+ a_{10} Issue3 + a_{11} Acquirer3 + a_{12} Target3 + a_{13} Divest3 \\ &+ a_{14} Cross\_List + a_{15} Foreign\_Sales_{-1} + \Sigma b_i Country_i \\ &+ \Sigma c_j Year_j + \Sigma d_k Industry_k). \end{aligned} \tag{1}$$

The dependent variable *Adopt* is equal to 1 for adopting firms, and 0 otherwise. All the independent variables are measured around event year 0 (detailed variable definitions are in the Appendix). The model also includes country, year, and industry dummy variables.

Following Bushman and Piotroski (2006) we argue that firms with more closely held shares face lower management compensation contracting demand on accounting earnings, thus a lower likelihood of IFRS/U.S. GAAP adoption. Furthermore, we predict that firms with lower labor productivity (measured by industry-adjusted sales per employee) face a greater need for informative measures of firm performance to facilitate internal performance evaluation, therefore a higher probability of IFRS/U.S. GAAP adoption. We include the percentage of closely held shares ($Close\_Held_0$) and labor productivity (industry-adjusted

TABLE 2
Descriptive Statistics

Panel A: Descriptive Statistics around Event Year Zero[a]

	Adopting Firms				Local Standards Firms			
	Mean	**Median**	**Std.**	**n**	**Mean**	**Median**	**Std.**	**n**
$Close\_Held_0$ (%)	50.4758***	51.0000***	25.1429	200	58.4130	59.9450	25.4589	766
$Labor\_Prod_{-1}$	41.0275*	2.5000	206.7219	200	83.1079	−1.3775	474.5006	766
RET_{-1}	0.0548	0.0078	0.4008	200	0.0924	0.0136	0.4634	766
ROA_{-1}	0.0691	0.0684	0.0738	200	0.0605	0.0693	0.1009	766
$Size_{-1}$	13.0609***	12.9642***	1.6165	200	11.4225	11.3126	1.5918	766
Lev_{-1}	0.1588***	0.1338***	0.1282	200	0.1251	0.1016	0.1200	766
$Growth_{-1}$	0.0894	0.0514	0.2450	200	0.0876	0.0472	0.3046	766
$Analyst_{-1}$	11.9800***	2.0000***	17.7274	200	5.2128	0.0000	10.4186	766
$Institution_{-1}$	0.0350**	0.0000***	0.1842	200	0.0052	0.0000	0.0721	766
Issue3	0.1650***	0.0000***	0.3721	200	0.0744	0.0000	0.2626	766
Acquirer3	0.4300***	0.0000***	0.4963	200	0.2272	0.0000	0.4193	766
Target3	0.0750	0.0000	0.2641	200	0.0705	0.0000	0.2561	766
Divest3	0.0200	0.0000	0.1404	200	0.0170	0.0000	0.1292	766
Cross-list	0.1350***	0.0000***	0.3426	200	0.0144	0.0000	0.1190	766
$Foreign\_Sales_{-1}$	0.5239***	0.5744***	0.2792	200	0.3325	0.2995	0.2946	766

Panel B: CEO Turnover Rates by Country over the Sample Period[b]

	Adopting	Local Standards	Total	Number of Firm-Year Obs.
Austria	13.16%	14.29%	13.56%	59
Denmark	12.50%	19.42%	18.49%	119
France	19.18%	11.74%	12.67%	1,160
Germany	17.39%	12.53%	14.46%	809
Italy	23.29%	12.50%	22.22%	81

(continued on next page)

TABLE 2 (continued)

	Adopting	Local Standards	Total	Number of Firm-Year Obs.
Switzerland	17.31%	10.11%	15.15%	297
	17.93%	12.37%	14.14%	2,525

Panel C: Employee Layoff Rates by Country over the Sample Period[c]

	Adopting	Local Standards	Total	Number of Firm-Year Obs.
Austria	18.33%	38.71%	25.27%	182
Belgium	19.32%	18.21%	18.44%	423
Denmark	17.12%	21.89%	20.97%	577
Finland	31.25%	27.59%	27.81%	525
France	22.50%	20.79%	20.95%	3,422
Germany	19.78%	32.46%	28.64%	3,006
Italy	30.51%	20.83%	29.62%	260
The Netherlands	19.61%	24.30%	24.06%	985
Sweden	31.34%	26.02%	26.48%	778
	21.87%	25.12%	24.50%	10,158

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*, **, *** Indicate that the mean or the median of the adopting firms is significantly different from that of the local standards firms at the 10 percent, 5 percent, or 1 percent levels, two-sided test, respectively.

[a] Event year zero is the IFRS/U.S. GAAP adoption year for adopting firms and a randomly selected year for local standards firms.

[b] This panel reports the percentage of firm-year observations experiencing a CEO turnover in each country.

[c] This panel reports the percentage of firm-year observations experiencing an employee layoff in each country. *Layoff* is a dichotomous variable as defined in the Appendix.

This table reports descriptive statistics of adopting and local standards firms around event year zero, CEO turnover rates by country, and employee layoff rates by country.

Variable definitions are in the Appendix.

sales per employee, $Labor\_Prod_{-1}$) in the regression and expect the coefficients on both variables to be negative.

Prior research suggests that variables associated with disclosure incentives have predictive power for the adoption decision (e.g., Harris and Muller 1999; Leuz and Verrecchia 2000; Ashbaugh 2001). We include lagged variables on firm performance (RET_{-1} and ROA_{-1}), firm size ($Size_{-1}$), leverage (Lev_{-1}), growth ($Growth_{-1}$), analyst following ($Analyst_{-1}$), and U.S. institutional holdings ($Institution_{-1}$) on the right-hand side of the regression model and expect the coefficients on firm size, leverage, growth, analyst following, and U.S. institutional holdings to be positive. We do not have predictions for stock returns and accounting returns on assets.[17]

We also include variables to measure, over the three-year period of event year 0 plus the following two years, firms' debt and equity issuance (*Issue3*), acquisition-related activities (*Acquirer3* and *Target3*), and divestitures (*Divest3*). We expect the coefficients on these variables to be positive as the adoption decision can reflect greater information demand from investors due to these corporate activities. Finally, to proxy for the information demand from foreign investors and other foreign stakeholders, we follow Ashbaugh (2001) and Leuz and Verrecchia (2000) and control for U.S. and U.K. cross-listings (*Cross_List* is equal to 1 if the firm is cross-listed in the U.S. or U.K. during our sample period). We also include foreign sales as a percentage of firm total sales ($Foreign\_Sales_{-1}$). We expect these variables to have positive signs.

The regression results are reported in Table 3. The coefficient estimates, standard errors, and the marginal effects are reported in columns (1) to (3), respectively. As predicted, $Close\_Held_0$ has a negative coefficient, -0.0089, and significant at the 0.05 level. The marginal effect suggests that a one standard deviation increase in the percentage of closely held shares decreases the adoption likelihood by 1.26 percent, or 6 percent of unconditional adoption probability of 20.7 percent (200/966). This supports our argument that the greater demand for more informative and conservative accounting earnings due to performance evaluations at more widely held firms increases these firms' incentives to adopt international accounting standards.[18] The coefficient on $Labor\_Prod_{-1}$ is -0.0010, negative as expected and significant at the 0.05 level. The marginal effect indicates that a one standard deviation increase in labor productivity reduces the likelihood of adoption by 2.16 percent, or roughly 10.4 percent of unconditional adoption probability of 20.7 percent (200/966). This suggests that firms with lower labor productivity compared to their industry peers have greater incentives to adopt international accounting standards. The findings on the control variables suggest that larger firms, those with higher leverage, with more acquisition activities, those that cross-list in the U.S. or U.K., and firms with more substantial foreign sales are more

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[17] The prediction regarding firm performance is ambiguous, as both good and bad performances can lead to more disclosure. For example, Lang and Lundholm (1993) and Miller (2002) document a positive relation between firm performance and disclosure and Skinner (1994) suggests that firms disclose bad news to reduce litigation risk.

[18] The percentage of closely held shares can also vary with firms' incentives to access the capital market as more closely held firms may have lower demand for external capital. This is the reason why we control for various factors related to firm financing needs in the regression model. To the extent the controls are adequate, our findings on *Close_Held* are consistent with compensation contracting demands affecting firms' decisions to adopt international accounting standards.

TABLE 3
Logistic Analysis of the IFRS/U.S. GAAP Adoption Decision

$$\begin{aligned} Prob\,[Adopt = 1] = Logit\,(&a_0 + a_1 Close\_Held_0 + a_2 Labor\_Prod_{-1} + a_3 RET_{-1} + a_4 ROA_{-1} \\ &+ a_5 Size_{-1} + a_6 Lev_{-1} + a_7 Growth_{-1} + a_8 Analyst_{-1} + a_9 Institution_{-1} \\ &+ a_{10} Issue3 + a_{11} Acquirer3 + a_{12} Target3 + a_{13} Divest3 + a_{14} Cross\_list \\ &+ a_{15} Foreign\_Sales_{-1} + \Sigma b_i Country_i + \Sigma c_j Year_j + \Sigma d_k Industry_k). \qquad (1) \end{aligned}$$

	Predicted sign	Estimate	Std. Error	Marginal Effects[a]
$Close\_Held_0$	–	−0.0089**	0.0052	−1.26%
$Labor\_Prod_{-1}$	–	−0.0010**	0.0005	−2.16%
RET_{-1}		−0.2269	0.2894	−0.60%
ROA_{-1}		−1.1219	1.4396	−0.63%
$Size_{-1}$		0.5319***	0.0921	8.43%
Lev_{-1}		2.6008***	0.9774	2.25%
$Growth_{-1}$		−0.5966	0.4041	−1.00%
$Analyst_{-1}$		−0.0002	0.0097	−0.01%
$Institution_{-1}$		0.3516	1.1336	2.51%
Issue3		0.3146	0.3809	2.16%
Acquirer3		0.7995***	0.2639	5.84%
Target3		−0.2508	0.4824	−1.41%
Divest3		−0.0306	0.9187	−0.19%
Cross-list		1.4282**	0.5680	15.49%
$Foreign\_Sales_{-1}$		2.4177***	0.4602	6.16%
Country Fixed Effects			Yes	
Year Fixed Effects			Yes	
Industry Fixed Effects			Yes	
Number of Adopting Firms			200	
Number of Local Standards Firms			766	
McFadden R^2			0.44	

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*, **, *** Indicate that a coefficient is significantly different from zero at the 10 percent, 5 percent, 1 percent levels, respectively (one-sided tests for coefficients with predictions and two-sided tests for those without a prediction).

[a] Marginal effects measure the changes in the predicted probability from a one standard deviation increase from the mean for a continuous variable and from 0 to 1 for an indicator variable with the other variables measured at the mean.

This table reports the logistic regression results to model firms' decisions to adopt IFRS/U.S. GAAP.

Variable definitions are in the Appendix.

likely to adopt IFRS/U.S. GAAP. The regression has reasonable predictive power with a Pseudo R^2 of 44 percent.[19]

V. CEO TURNOVER

Hypothesis 1 predicts that the sensitivity of CEO turnover to accounting earnings increases after the adoption of international accounting standards. We include stock returns as a second performance measure because prior studies show that both earnings and stock returns affect management turnover (see, for example, Kaplan [1994], Franks and Mayer [2001], and Volpin [2002] for international evidence). Following the methodology in prior studies, such as Kaplan (1994) and Franks and Mayer (2001), we define an indicator variable *DROA* as 1 if accounting earnings (measured by net income before extraordinary items) in a firm-year is negative, and 0 otherwise; and define an indicator variable *DRET* as 1 if the stock return in a firm-year is below −20 percent, and 0 otherwise. Among the 2,525 firm-year observations included in the management turnover tests, about 9 percent are with earnings losses (*DROA* = 1) and 17 percent have stock returns below −20 percent (*DRET* = 1).

We first analyze CEO turnover-to-performance sensitivities separately for the adopting and the local standards samples with model (2) below (firm subscripts are suppressed).

$$\begin{aligned} Prob\,[CEO\_Turnover_t = 1] = {} & Logit\,(a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} \\ & + a_3 Post + a_4 Post{*}DROA_{t-1} \\ & + a_5 Post{*}DRET_{t-1} + \Sigma b_j Control\ variable_j). \qquad (2) \end{aligned}$$

The dependent variable, $CEO\_Turnover_t$, is an indicator equal to 1 if there is a CEO turnover in year *t*, and 0 otherwise. *Post* is an indicator variable, equal to 1 if the firm-year is post-event year 0, and 0 otherwise (event year 0 itself is removed from the analysis). We include the explanatory variables from our earlier adoption decision regression (except for *ROA* and *RET*) to control for firms' incentives to adopt international accounting standards and their potential impact on CEO turnover. These variables are measured around year *t*. We also include country, year, and industry dummy variables in the regression. Statistical tests are conducted using robust standard errors clustered by firm.

Hypothesis 1 predicts that the CEO turnover sensitivity to poor accounting performance increases after the adoption of international accounting standards. We therefore expect a positive coefficient on $Post{*}DROA_{t-1}$ for the adopting sample, while we do not expect the same for the local standards firms, for which event year 0 is randomly selected. The results for model (2) are reported in Panel A of Table 4. Consistent with H1, $Post{*}DROA_{t-1}$ has a positive coefficient that is significant at the 0.01 level for the adopting sample. On the other hand, $Post{*}DROA_{t-1}$ is insignificantly different from zero for the local standards sample. We do not have specific predictions regarding stock returns, although the insignificant coefficient on $Post*DRET_{t-1}$ is inconsistent with an *overall* increase in the performance sensitivities of CEO turnover at the adopting firms that might result from concurrent organizational changes other than the accounting changes.

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[19] Leuz and Verrecchia (2000) obtain an R^2 of 40 percent in their adoption decision regression and Ashbaugh (2001) reports an 18 percent R^2 in her study. Regarding the significance of the individual explanatory variables in the adoption decision regression, while the findings in prior studies are consistent with the overall theme that potential informational benefits drive the adoption of international accounting standards, the results on individual variables vary across studies, likely due to differences in sample selection and methodology. For example, Leuz and Verrecchia (2000) find a positive coefficient for ROA; while Harris and Muller (1999) do not find such evidence. Harris and Muller (1999) document a positive role for firm size in the adoption decision; whereas Leuz and Verrecchia (2000) find an insignificant coefficient for size. Harris and Muller (1999) report insignificant coefficients on debt and equity issuance, although Ashbaugh (2001) finds a positive role of equity issuance in the adoption decision.

To statistically assess the differences reported in Panel A between the adopting and local standards firms, we combine the two samples in a "difference-in-differences" test based on model (2′) below and report the results in Panel B of Table 4.

TABLE 4
CEO Turnover-to-Performance Sensitivity Analysis

Panel A: CEO Turnover-to-Performance Sensitivity Pre- and Post-Event Year Zero for Adopting and Local Standards Firms Separately[a]

$$\begin{aligned} Prob\ [CEO\_Turnover_t = 1] = Logit\ (&a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Post \\ &+ a_4 Post*DROA_{t-1} + a_5 Post*DRET_{t-1} \\ &+ \Sigma b_j Control\ variable_j) \end{aligned} \tag{2}$$

		Adopting		Local Standards	
	Predicted Sign	Estimate	Std. Error	Estimate	Std. Error
$DROA_{t-1}$	+	−0.5222	0.4939	0.4495	0.4100
$DRET_{t-1}$	+	0.0441	0.4899	0.6007**	0.3344
$Post_t$		−0.0830	0.2912	0.0220	0.1875
$Post_t*DROA_{t-1}$	+ (Adopting sample only)	1.6124***	0.6194	−0.0359	0.4947
$Post_t*DRET_{t-1}$		0.1620	0.5949	−0.1415	0.3971
$Close\_Held_t$		0.0014	0.3930	0.0053	0.3870
$Labor\_Prod_{t-1}$		−0.0001	0.0004	0.0001	0.0001
$Size_{t-1}$		0.1715**	0.0812	0.0792	0.0671
Lev_{t-1}		−1.0239	1.0145	0.0792	0.6124
$Growth_{t-1}$		−0.4314	0.8127	−0.8056*	0.4599
$Analyst_{t-1}$		−0.0071	0.0058	−0.0002	0.0077
$Institution_{t-1}$		0.1770	0.3812	−0.7720	0.8946
Issue3		−0.5136**	0.2312	−0.1203	0.3339
Acquirer3		0.2392	0.2054	−0.0483	0.2270
Target3		−0.2418	0.3713	0.5842**	0.2844
Divest3		−0.9779	0.9748	−0.1998	0.4895
Cross-list		−0.7291**	0.3273	−0.5278	0.5834
$Foreign\_Sales_{t-1}$		−0.3898	0.4184	−0.0468	0.3521
Country Fixed Effects		Yes		Yes	
Year Fixed Effects		Yes		Yes	
Industry Fixed Effects		Yes		Yes	
Number of Obs with CEO Turnover		144		213	
Number of Obs without CEO Turnover		659		1509	
MacFadden R^2		0.05		0.05	

(*continued on next page*)

TABLE 4 (continued)

Panel B: CEO Turnover-to-Performance Sensitivity Pre- and Post-Event Year Zero for the Pooled Sample[b]

$$\begin{aligned} Prob\,[CEO\_Turnover_t = 1] = Logit(&a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Adopt \\ &+ a_4 Adopt{*}DROA_{t-1} + a_5 Adopt{*}DRET_{t-1} + a_6 Post \\ &+ a_7 Post{*}DROA_{t-1} + a_8 Post{*}DRET_{t-1} \\ &+ a_9 Post{*}Adopt + a_{10} Post{*}Adopt{*}DROA_{t-1} \\ &+ a_{11} Post{*}Adopt{*}DRET_{t-1} + \Sigma b_j Control\ variable_j) \qquad (2') \end{aligned}$$

	Predicted Sign	Estimate	Std. Error
$DROA_{t-1}$	+	0.4495	0.4115
$DRET_{t-1}$	+	0.6007**	0.3356
$Adopt$		1.3759	1.7683
$Adopt{*}DROA_{t-1}$		−0.9718	0.6391
$Adopt{*}DRET_{t-1}$		−0.5566	0.5898
$Post_t$		0.0220	0.1882
$Post_t{*}DROA_{t-1}$		−0.0359	0.4965
$Post_t{*}DRET_{t-1}$		−0.1415	0.3986
$Post_t{*}Adopt$		−0.1050	0.3443
$Post_t{*}Adopt{*}DROA_{t-1}$	+	1.6483**	0.7891
$Post_t{*}Adopt{*}DRET_{t-1}$		0.3035	0.7112
$Close\_Held_t$		0.0053	0.0039
$Labor\_Prod_{t-1}$		0.0001	0.0001
$Size_{t-1}$		0.0792	0.0673
Lev_{t-1}		0.0792	0.6146
$Growth_{t-1}$		−0.8056*	0.4616
$Analyst_{t-1}$		−0.0002	0.0077
$Institution_{t-1}$		−0.7720	0.8979
Issue3		−0.1203	0.3351
Acquirer3		−0.0483	0.2278
Target3		0.5842**	0.2855
Divest3		−0.1998	0.4913
Cross-list		−0.5278	0.5856
$Foreign\_Sales_{t-1}$		−0.0468	0.3534
Interaction of *Adopt* with Control Variables		Yes	
Country Fixed Effects		Yes	
Year Fixed Effects		Yes	
Industry Fixed Effects		Yes	
Number of Obs with CEO Turnover		357	
Number of Obs without CEO Turnover		2168	
MacFadden R^2		0.06	

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*, **, *** Indicate that a coefficient is significantly different from zero at the 10 percent, 5 percent, 1 percent levels, respectively (one-sided tests for coefficients with predictions and two-sided tests for those without a prediction).

[a] This panel reports the estimation results of model (2).

(*continued on next page*)

TABLE 4 (continued)

[b] This panel reports the estimation results of model (2′).

This table reports the changes in CEO turnover-to-performance sensitivities around IFRS/U.S. GAAP adoption.

Control variables are the explanatory variables from the adoption prediction regression (except for *ROA* and *RET*).

Variable definitions are in the Appendix.

$$\begin{aligned} Prob\,[CEO\_Turnover_t = 1] = Logit\,(&a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} \\ &+ a_3 Adopt + a_4 Adopt{*}DROA_{t-1} \\ &+ a_5 Adopt{*}DRET_{t-1} + a_6 Post \\ &+ a_7 Post{*}DROA_{t-1} + a_8 Post{*}DRET_{t-1} \\ &+ a_9 Post{*}Adopt + a_{10} Post{*}Adopt{*}DROA_{t-1} \\ &+ a_{11} Post{*}Adopt{*}DRET_{t-1} \\ &+ \Sigma b_j Control\ variable_j). \end{aligned} \tag{2'}$$

Adopt is an indicator equal to 1 for firms in the adopting sample, and 0 for the local standards sample. To allow all coefficients from model (2) to vary with *Adopt*, the control variables in model (2) are also interacted with it. The coefficients a_1, on $DROA_{t-1}$, and a_2, on $DRET_{t-1}$, measure the turnover sensitivity to poor accounting performance and poor stock performance, respectively, for the "base category" of *local standards firms prior to event year 0.* The significant and positive coefficient on $DRET_{t-1}$ suggests that CEO turnover is sensitive to poor stock performance. The coefficient on $DROA_{t-1}$ is positive, although insignificant. Overall, these results are in line with the inferences from prior studies that the governance systems in Continental Europe discipline poorly performing managers (e.g., Kaplan 1994; Franks and Mayer 2001; Volpin 2002).

The coefficients on $Adopt{*}DROA_{t-1}$ and $Adopt{*}DRET_{t-1}$ measure the incremental sensitivities of CEO turnover to poor accounting and stock performances at the *adopting firms prior to their adoption* of international accounting standards. Neither coefficient is significantly different from zero, indicating that the adopting and local standards firms have no differential sensitivities of CEO turnover to poor performance before event year 0.[20] The coefficients on $Post{*}DROA_{t-1}$ and $Post{*}DRET_{t-1}$ measure the incremental sensitivities of CEO turnover to poor accounting and stock performances at the *local standards firms post event year 0.* Neither coefficient is significantly different from zero, suggesting the turnover-to-performance sensitivities do not change for the local standards firms from before to after event year 0, consistent with our expectations.

The coefficient on the triple interactive term, $Post{*}Adopt{*}DROA_{t-1}$, measures the difference between the adopting and local standards samples, in the changes of their turnover-to-poor accounting performance sensitivities from pre- to post-event year 0. The coefficient on $Post{*}Adopt{*}DRET_{t-1}$ captures the corresponding difference-in-differences in the sensitivity of CEO turnover to poor stock performance. Hypothesis 1 predicts a positive coefficient on $Post{*}Adopt{*}DROA_{t-1}$, i.e., the increase in the sensitivity of CEO turnover to poor accounting performance at the adopting firms from pre- to post-adoption is greater than that experienced by the local standards firms. The positive coefficient on $Post*Adopt*DROA_{t-1}$, which is significant at the 0.05 level, supports H1 and corroborates the evidence reported in Table 4, Panel A that at the adopting firms CEO turnover is more responsive to poor accounting performance post-adoption. The insignificant coefficient on $Post*Adopt*DRET_{t-1}$ suggests that the changes in the sensitivities of CEO turnover to stock performance from pre- to post-event year 0 do not differ between the adopting and local standards samples. On the control variables, we find that target firms (*Target3*) experience more frequent CEO turnover; while CEO turnover rates are lower for high-growth firms ($Growth_{t-1}$).[21] The R^2 from the regression is around 6 percent, in line with prior studies of European firms (e.g., Volpin 2002).

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[20] The earlier results in Table 4, Panel A seem to suggest that prior to adoption, the adopting firms have lower performance sensitivities in CEO turnover than local standards firms. However, the difference-in-differences analysis in Panel B of Table 4 does not suggest a *statistically significant* difference between the two groups, i.e., neither $Adopt{*}DROA_{t-1}$ nor $Adopt{*}DRET_{t-1}$ are significant.

VI. EMPLOYEE LAYOFFS

Our second hypothesis involves the sensitivities of employee layoffs to accounting earnings before and after the adoption of international accounting standards. Similar to the CEO turnover analysis, we use *DROA* and *DRET* (defined in the previous section) as performance measures for accounting earnings and stock returns, respectively. Among the 10,158 firm-year observations included in the layoff tests, about 10 percent are with earnings losses ($DROA = 1$) and 22 percent have stock returns below -20 percent ($DRET = 1$).

We first conduct an analysis of layoff-to-performance sensitivities separately for the adopting and local standards samples with model (3) below (firm subscripts are suppressed).

$$
\begin{aligned}
Prob\ [Layoff_t = 1] = \text{Logit}\ (&a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Post \\
&+ a_4 Post*DROA_{t-1} + a_5 Post*DRET_{t-1} \\
&+ \Sigma b_j Control\ variable_j)\,. \qquad (3)
\end{aligned}
$$

The dependent variable, $Layoff_t$, is an indicator, equal to 1 if there is a reduction of a firm's employee headcount of more than 5 percent in year *t*, and 0 otherwise. The explanatory variables on the right-hand side are the same as those in model (2) on management turnover, except for the addition of several control variables. Since the change in employee headcount can reflect *contemporaneous* changes in a firm's overall scale of operations, we include sales growth (*Growth*), change in foreign sales ($\Delta Foreign\_Sales$), and an indicator variable for fixed assets disposal (*Fix_Disposal*), for year *t*. Continental European countries are known for their strong employment protection laws and powerful labor unions (e.g., Grubb and Wells 1993; Nicoletti et al. 1999), as a result, we also include a variable on the intensity of union activities (*Union*).[22] Similar to the CEO turnover analysis, statistical tests are conducted using robust standard errors clustered by firm.

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[21] The lack of significance for the other control variables should not cause concern because they are included to control for firms' incentives to adopt international accounting standards. Performance measures (accounting earnings and stock returns) are the primary explanatory variables of CEO turnover in our study, as is the case in prior research (e.g., Warner et al. 1988; Engel et al. [2003] with U.S. data; Kaplan [1994] and DeFond and Hung [2004] in the international setting). Model (2′) includes the interactive terms of *Adopt* with the control variables. To save space, their coefficients are not reported in Table 4. Most of the interactive terms are insignificant, except for *Adopt*Target3*, which has a marginally significant negative coefficient. Detailed regression results are available upon request from the authors.

Hypothesis 2 predicts that the employee layoff sensitivity to poor accounting performance increases after the adoption of international accounting standards. We therefore expect a positive coefficient on $Post{*}DROA_{t-1}$ for the adopting sample, while we do not expect the same for the local standards firms. The results for model (3) are reported in Panel A of Table 5. Consistent with H2, $Post{*}DROA_{t-1}$ has a positive coefficient, which is significant at the 0.05 level, for the adopting sample. On the other hand, $Post{*}DROA_{t-1}$ is not significantly different from zero for the local standards sample. We do not have specific predictions regarding stock returns, although the insignificant coefficient on $Post{*}DRET_{t-1}$ is inconsistent with an *overall* increase in the performance sensitivities of employee layoffs at the adopting firms that might result from concurrent organizational changes other than the accounting changes.

To statistically assess the differences reported in Table 5, Panel A between the adopting and local standards firms, we combine the two samples in a "difference-in-differences" test based on model (3′) below and report the results in Panel B of Table 5.

$$\begin{aligned} Prob\,[Layoff_t = 1] = {} & Logit\,(a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Adopt \\ & + a_4 Adopt{*}DROA_{t-1} + a_5 Adopt{*}DRET_{t-1} + a_6 Post \\ & + a_7 Post{*}DROA_{t-1} + a_8 Post{*}DRET_{t-1} \\ & + a_9 Post{*}Adopt + a_{10} Post{*}Adopt{*}DROA_{t-1} \\ & + a_{11} Post{*}Adopt{*}DRET_{t-1} + \Sigma b_j Control\ variable_j). \qquad (3') \end{aligned}$$

Adopt is an indicator, equal to 1 for firms in the adopting sample, and 0 for the local standards sample. To allow all coefficients from model (3) to vary with *Adopt*, the control variables in model (3) are also interacted with it. The coefficients a_1, on $DROA_{t-1}$, and a_2, on $DRET_{t-1}$, measure the layoff sensitivity to poor accounting performance and poor stock performance, respectively, for the "base category" of *local standards firms prior to event year 0*. The significant and positive coefficients on both $DROA_{t-1}$ and $DRET_{t-1}$ suggest that employee layoffs are responsive to poor firm performance. The coefficients on $Adopt{*}DROA_{t-1}$ and $Adopt{*}DRET_{t-1}$ are insignificant, indicating the adopting and local standards firms have no differential sensitivities of employee layoffs to poor performance before event year 0. The coefficients on $Post{*}DROA_{t-1}$ and $Post{*}DRET_{t-1}$ are also insignificant, suggesting the layoff-to-performance sensitivities do not change for the local standards firms from before to after event year 0, consistent with our expectations.

Hypothesis 2 predicts a positive coefficient on $Post{*}Adopt{*}DROA_{t-1}$, i.e., the increase in the sensitivity of employee layoffs to poor accounting performance at the adopting firms from pre- to post-adoption is greater than that experienced by the local standards firms. The positive coefficient on $Post{*}Adopt{*}DROA_{t-1}$, which is significant at the 0.05 level, supports H2, and corroborates the evidence reported in Table 5, Panel A that at the adopting firms, employee layoffs are more responsive to poor accounting performance post-adoption. The insignificant coefficient on $Post*Adopt*DRET_{t-1}$ suggests that the changes in the sensitivities of employee layoffs to stock performance from pre- to post-event year 0 do not differ between the adopting and local standards samples. On the control variables, we find

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[22] Union intensity information is obtained from the European Social Survey Round One (Jowell et al. 2003), based on surveys of 42,359 individuals from European Union countries over the period of 2002–2003. Participants of the survey were asked whether they were a member of a trade union in the past 12 months. We measure union density for each one-digit SIC industry in each country with the percentage of participants indicating they are trade union members. The resulting measure of union intensity is at the country-industry level. Because the information on union intensity is available at only one point in time (2002–2003), we apply it to our entire sample period under the assumption that union participation levels change slowly overtime.

TABLE 5
Employee Layoff-to-Performance Sensitivity Analysis

Panel A: Employee Layoff-to-Performance Sensitivity Pre- and Post-Event Year Zero for Adopting and Local Standards Firms Separately[a]

$$Prob\ [Layoff_t = 1] = Logit\ (a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Post + a_4 Post*DROA_{t-1} + a_5 Post*DRET_{t-1} + \Sigma b_j Control\ variable_j) \qquad (3)$$

		Adopting		Local	
	Predicted Sign	Estimate	Std. Error	Estimate	Std. Error
$DROA_{t-1}$	+	0.5710*	0.3677	1.0252***	0.1688
$DRET_{t-1}$	+	0.4031**	0.2100	0.2770**	0.1185
$Post_t$		0.0539	0.2324	0.0773	0.0867
$Post_t*DROA_{t-1}$	+ (Adopting sample only)	1.0670**	0.5256	0.0069	0.1947
$Post_t*DRET_{t-1}$		0.3936	0.2806	0.1566	0.1364
$Close\_Held_t$		0.0066*	0.3870	0.0019	0.1530
$Labor\_Prod_{t-1}$		−0.0012	0.0008	−0.0002**	0.0001
$Size_{t-1}$		−0.0354	0.0579	−0.0549**	0.0262
Lev_{t-1}		0.7957	0.7662	0.6587**	0.2717
$Growth_{t-1}$		−0.2533	0.4230	−0.6068***	0.1736
$Analyst_{t-1}$		0.0017	0.0049	0.0058*	0.0031
$Institution_{t-1}$		−0.8532*	0.4644	−0.0586	0.2614
$Issue3$		0.1455	0.2135	−0.0327	0.1176
$Acquirer3$		−0.2395	0.1743	−0.3042***	0.0797
$Target3$		0.1932	0.2798	−0.0008	0.1280
$Divest3$		−0.1204	0.4763	0.5092**	0.2436
$Cross\text{-}list$		−0.1076	0.2474	0.3250	0.2240
$Foreign\_Sales_{t-1}$		−0.1126	0.3093	0.0708	0.1260
$\Delta Foreign\_Sales_t$		−0.5262	1.2439	−0.6732	0.5366
$Growth_t$		−8.3593***	1.0196	−4.7546***	0.3563
$Fix\_Disposal_t$		−0.0172	0.2132	0.0669	0.0724
$Union$		−1.3019	1.9165	0.9336*	0.5658
Country Fixed Effects		Yes		Yes	
Year Fixed Effects		Yes		Yes	
Industry Fixed Effects		Yes		Yes	
Number of Obs with Employee Layoffs		422		2067	
Number of Obs without Layoffs		1508		6161	
MacFadden R^2		0.27		0.17	

(*continued on next page*)

TABLE 5 (continued)

Panel B: Employee Layoff-to-Performance Sensitivity Pre- and Post-Event Year Zero for the Pooled Sample[b]

$$\begin{aligned} Prob\,[Layoff_t = 1] = Logit\,(&a_0 + a_1 DROA_{t-1} + a_2 DRET_{t-1} + a_3 Adopt \\ &+ a_4 Adopt{*}DROA_{t-1} + a_5 Adopt{*}DRET_{t-1} + a_6 Post \\ &+ a_7 Post{*}DROA_{t-1} + a_8 Post{*}DRET_{t-1} + a_9 Post{*}Adopt \\ &+ a_{10} Post{*}Adopt{*}DROA_{t-1} + a_{11} Post{*}Adopt{*}DRET_{t-1} \\ &+ \Sigma b_j Control\ variable_j) \end{aligned} \tag{3'}$$

	Predicted Sign	Estimate	Std. Error
$DROA_{t-1}$	+	1.0252***	0.1692
$DRET_{t-1}$	+	0.2770**	0.1187
Adopt		2.2454	2.1641
$Adopt{*}DROA_{t-1}$		−0.4542	0.4017
$Adopt{*}DRET_{t-1}$		0.1261	0.2396
$Post_t$		0.0773	0.0869
$Post_t{*}DROA_{t-1}$		0.0069	0.1951
$Post_t{*}DRET_{t-1}$		0.1566	0.1366
$Adopt{*}Post_t$		−0.0234	0.2461
$Post_t{*}Adopt{*}DROA_{t-1}$	+	1.0602**	0.5561
$Post_t{*}Adopt{*}DRET_{t-1}$		0.2370	0.3098
$Close\_Held_t$		0.0019	0.0015
$Labor\_Prod_{t-1}$		−0.0002**	0.0001
$Size_{t-1}$		−0.0549**	0.0263
Lev_{t-1}		0.6587**	0.2722
$Growth_{t-1}$		−0.6068***	0.1739
$Analyst_{t-1}$		0.0058**	0.0031
$Institution_{t-1}$		−0.0586	0.2619
Issue3		−0.0327	0.1178
Acquirer3		−0.3042***	0.0798
Target3		−0.0007	0.1283
Divest3		0.5092**	0.2441
Cross-list		0.3250	0.2245
$Foreign\_Sales_{t-1}$		0.0708	0.1262
$\Delta Foreign\_Sales_t$		−0.6732	0.5376
$Growth_t$		−4.7549***	0.3570
$Fix\_Disposal_t$		0.0669	0.0725
Union		0.9336*	0.5668
Interaction of *Adopt* with Control Variables		Yes	
Country Fixed Effects		Yes	
Year Fixed Effects		Yes	
Industry Fixed Effects		Yes	
Number of Obs with Employee Layoffs		2489	
Number of Obs without Layoffs		7669	
MacFadden R^2		0.19	

(continued on next page)

TABLE 5 (continued)

*, **, *** Indicate that a coefficient is significantly different from zero at the 10 percent, 5 percent, 1 percent levels, respectively (one-sided tests for coefficients with predictions and two-sided tests for those without a prediction).

[a] This panel reports the estimation results of model (3).

[b] This panel reports the estimation results of model (3′).

This table reports the changes in employee layoff-to-performance sensitivities around IFRS/U.S. GAAP adoption.

Control variables include the explanatory variables from the adoption prediction regression (except for *ROA* and *RET*) and additional variables measuring contemporaneous changes in a firm's overall scale of operations and union intensity.

Variable definitions are in the Appendix.

that firms with higher labor productivity, that are larger, with greater contemporaneous and lagged sales growth, and with more acquisition activities experience less frequent layoffs. On the other hand, firms with higher leverage, more analyst following, and with divestitures have more frequent employee layoffs.[23,24]

VII. ROBUSTNESS TESTS

Cross-Sectional and Inter-Temporal Variations in the Effects of IFRS/U.S. GAAP Adoption

DeFond and Hung (2004) document that the association between CEO turnover and poor performance varies with the strength of country-level law enforcement institutions. If strong law enforcement facilitates the improvement in firms' internal performance evaluations post-IFRS/U.S. GAAP adoption, then we could observe a larger increase in earnings performance sensitivities at the adopting firms in countries with stronger law enforcement. We classify our sample countries into those with strong and weak law enforcement based on the sample median and set an indicator variable to 1 for countries with stronger law enforcement, and 0 otherwise.[25] We interact it with the relevant variables in models (2′) and (3′). Untabulated results suggest that the increase in the earnings sensitivities of employee layoffs post-adoption is significantly higher in countries with stronger law enforcement. However, we do not find the increase in the earnings sensitivities of CEO turnover post-adoption to vary with the strength of law enforcement. [26] The low variation in the law enforcement score among Continental European countries can reduce our test power (see footnote 25).

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[23] The union variable has a positive coefficient that is marginally significant. While the presence of union can protect employment, thus lowering the frequency of layoffs, as pointed out by Cioffi (2002), union representatives, who often have seats on the supervisory board in countries such as Germany, Denmark, The Netherlands, and Sweden, may also facilitate restructuring by providing the rank and file employees "with sufficient information on the state of the business to justify drastic measures."

[24] Most of the interactive terms between *Adopt* and the control variables are insignificant, except for $Adopt*Growth_t$, which has a negative coefficient, significant at the 0.01 level. Detailed regression results are available upon request from the authors.

[25] The law enforcement index for each country is calculated as the mean of the five legal enforcement variables in LaPorta et al. (1998): (1) efficiency of the judicial system; (2) rule of law; (3) corruption; (4) risk of expropriation; and (5) risk of contract repudiation. The mean score among our sample countries is 9.4 (median of 9.6, out of a possible 10) and varying from 7.9 (Italy, the lowest) to 9.9 (Switzerland, the highest). Since our sample countries are from Continental Europe, the variation in the score is small (standard deviation of 0.6, compared to 2.15 in DeFond and Hung [2004]).

[26] For brevity, the results on these and other robustness tests in Section VII are not tabulated. They are available upon request from the authors.

We further investigate whether the adoption of IFRS/U.S. GAAP has a transitory or a more persistent impact on firm internal performance evaluations. We create an indicator variable for the three-year period immediately after event year 0 and a separate indicator for the period after that. These variables replace the single post-event year 0 indicator, *Post*, in models (2′) and (3′). Untabulated findings suggest that the increases in earnings performance sensitivities in both CEO turnover and employee layoffs persist beyond the initial post-adoption period, although such effects are stronger in the three years immediately after adoption.

Alternative Variable Specifications

Thus far we measure earnings and stock performances with indicator variables of negative ROA (*DROA*) and stock returns below −20 percent (*DRET*), respectively. As a robustness check, we replace the indicators with continuous measures of ROA and stock returns. The inferences on employee layoffs are unaffected. However, the results on CEO turnover are sensitive to this change in variable specification. This suggests that the increase in the sensitivity of CEO turnover to accounting performance post-adoption is primarily driven by heightened turnover sensitivity to accounting *losses*.

To investigate if *increases* in employee headcount also become more sensitive to accounting earnings post-adoption, we construct an indicator for large hiring increases (5 percent or more *increase* in employee headcount) and substitute it for the layoff indicator in our analysis. Even though labor increases are positively associated with good performance (performance is measured either continuously or with good performance indicators), the sensitivity is not significantly higher for adopting firms post-adoption. It suggests that IFRS/U.S. GAAP earnings primarily facilitate the identification and restructuring of underperforming units.

Other Robustness Tests

We control for the effect of macro-economic conditions on CEO turnover and employee layoffs by including the market return in each country and year as a control variable in all regression models. The coefficients on market returns are insignificant in the various regressions and our main inferences remain unchanged. To account for any potential time trend in CEO turnover and employee layoffs, we include a time trend variable and interact it with earnings performance and stock returns in models (2′) and (3′). The inferences on our main test variables are not affected. Finally, to investigate the timing of the IFRS/U.S. GAAP adoption, we analyze the changes in labor productivity at the adopting firms. A test where we regress labor productivity against a time trend variable does not show a significant decreasing trend in labor productivity over event years [−5, 0]. It could be that firms' labor productivity is persistently low, not necessarily deteriorating continuously, in the several years leading up to the adoption. Meanwhile, there is a significant increase in labor productivity over event years [0, +5].

VIII. CONCLUSIONS

While a large body of research is devoted to understanding the causes and consequences of the adoption of international accounting standards, researchers' attention has thus far focused almost exclusively on the informational benefits of the adoption. We extend the existing literature by offering a different, stewardship perspective. We hypothesize that the voluntary adoption of international accounting standards is associated with changes in the internal performance evaluation process; in particular, it is associated with increases in the sensitivities of CEO turnover and employee layoffs to accounting earnings. These predictions are built on prior literature's evidence that accounting earnings are timelier, less managed, and more conservative after the adoption of international accounting standards. As a result, they are more effective tools for firm internal performance evaluations and governance. We test our hypotheses using a sample of Continental European firms and find evidence consistent with these predictions.

Our study is an application of the contracting-related perspective to the fast-growing area of international accounting research. We contribute to the literature by providing support for the long-held notion that stewardship demands interact with firms' accounting choices (e.g., Watts and Zimmerman 1986; Ball 2001; Watts 2003a; Bushman et al. 2006; O'Connell 2007).

We caution against inferring from our findings strict causality between the voluntary adoption of internationally recognized accounting standards and changes in firm internal performance evaluations. Both can be a part of the firms' broader strategies to improve transparency and corporate governance. However, it is important to document the association between the two phenomena. The evidence in this study adds richness to our understanding of firms' voluntary adoption decision.

APPENDIX
VARIABLE DEFINITIONS

Dependent and Explanatory Variables in the Adoption Decision Analysis

The explanatory variables in the adoption decision analysis are measured at the end of event year −1 unless stated otherwise in the following description table. These explanatory variables are then included in the management turnover and employee layoffs analyses as control variables. In the turnover and layoffs analyses, these variables are measured at the end of event year $t-1$, unless stated otherwise.

Variables	Definition
Adopt	1 if a firm adopts IFRS or U.S. GAAP during our sample period; 0 if a firm reports under local accounting standards throughout the sample period
Close_Held	Percentage of closely held shares at the end of event year 0 (event year t for the management turnover and employee layoffs analyses)
Labor_Prod	Labor productivity (sales per employee) minus the median labor productivity in the same industry group
RET	Annual raw stock return
ROA	Return on assets, accounting earnings is defined as net income before extraordinary items
Size	Natural logarithm of market capitalization in Euros as reported in Worldscope
Lev	Leverage, defined as long-term debt divided by total assets
Growth	Sales growth, current year's sales change divided by prior year's sales
Analyst	Number of analysts following a firm
Institution	1 if U.S. institutional holdings are nonzero, and 0 otherwise
Issue3	1 if there is new equity or debt issuance in the event year window [0, 2] (event year window $[t, t+2]$ for the management turnover and employee layoffs analyses), and 0 otherwise
Acquirer3	1 if a firm acquires another firm in the event year window [0, 2] (event year window $[t, t+2]$ for the management turnover and employee layoffs analyses), and 0 otherwise

(*continued on next page*)

Variables	Definition
Target3	1 if a firm is acquired in the event year window [0, 2] (event year window [t, t+2] for the management turnover and employee layoffs analyses), and 0 otherwise
Divest3	1 if there is a divestiture (including sales of divisions, spin-offs, split-offs, and carve-outs) in the event year window [0, 2] (event year window [t, t +2] for the management turnover and employee layoffs analyses); 0 otherwise
Cross-list	1 if a firm is cross-listed in the U.S. or the U.K. in the sample period, and 0 otherwise
Foreign_Sales	Foreign sales divided by total sales
Additional Variables in the CEO Turnover Analysis	
CEO_Turnover	1 if there is a CEO turnover in event year t; 0 otherwise.
DROA	1 if ROA of event year $t-1$ is negative, and 0 otherwise
DRET	1 if annual raw stock return of event year t–1 is less than –20%, and 0 otherwise
Post	1 if a firm-year observation is post-event year 0, and 0 for pre-event year 0 observations (event year 0 itself is removed).
Additional Variables in the Employee Layoffs Analysis	
Layoff	1 if the percentage of change in the number of employees in event year t is less than −5%, and 0 otherwise
Δ*Foreign_Sales*	Change in *Foreign_Sales* in year t (*Foreign_Sales* is defined as above)
Fix_Disposal	1 if fixed assets disposal in year t is nonzero, and 0 otherwise
Union	Percentage of European Social Survey Round One participants that indicate they are trade union members in each one-digit SIC industry and each country

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Management, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

A. NACIRI (editor), *Corporate Governance Around the World* (London, U.K.: Routledge, 2008, ISBN 10: 0-415-42874-2(hbk), pp. xix, 407).

An Internet search of the title *Corporate Governance Around the World* produced 2,720,000 hits relating to numerous publications with the same title or similar titles. Clearly, much has been written under this title, highlighting the popularity of the subject area of this book. The current publication, however, stands out from the others. The first few chapters of the book cover a wide range of theoretical perspectives surrounding corporate governance issues and do not simply focus on the agency theory's principal-agent corporate governance model. In this respect, this book has more in common with an earlier book that I edited (*Corporate Governance, Accountability and Pressures to Perform: An International Study*), which also adopted a theory-driven analysis of corporate governance practices in a number of industrialized countries around the world.

The contents include an introduction to the structure of the book and its rationale written by the editor of the book, Ahmed Naciri. Chapters 2 and 3 are contributed by Gérard Charreaux, who explores the theoretical aspects of corporate governance. Chapters 4 and 5 cover the American and Canadian national systems of corporate governance. Chapter 6 explores the French system, followed by the Japanese national system, of corporate governance. Chapters 8 and 9 refer to Hong Kong and the Chinese national systems. Then the Turkish national system of corporate governance is examined (although the rationale for its inclusion here is not clear, as Turkey is an Associate and an applicant member country of the European Union), followed by a chapter on the transition national systems of corporate governance. In Chapter 12, the Middle Eastern and North African (MENA) countries are examined, and finally Chapter 13 considers the European Union (EU) countries' corporate governance systems. The concluding section is written by the editor of the book.

In Chapter 2, Charreaux explores the micro theories of corporate governance and focuses on the creation and allocation of value associated with a theory of organization that is based on efficiency. He highlights the concept of value creation in the context of corporate governance based on the contributions from disciplinary and knowledge-based paradigms. He questions the compatibility of both paradigms and argues that Lezonick and O'Sullivan's (1998, 2000) "governance of the innovative firm" and the more ambitious works of Aoki's (1990, 2000) "theories of cooperative firm" may be considered as attempts at a study for constructing a theory of corporate governance where disciplinary and cognitive aspects are simultaneously apparent.

In Chapter 3, Charreaux synthesizes macro theories of corporate governance derived from relatively separate fields. This chapter provides a useful base for understanding the nature of firm, its development, and how and why the various forms of corporate governance systems will evolve in the future. One field covered in the chapter reflects the disciplinary perspective of firms based on organizational rents and the protection of the investors. The other are the legal and the financial fields. A number of political theories then follow that explain the configuration of the American national system. Finally, the endowment and the socio-cultural theories are briefly discussed within the context of corporate governance systems.

Chapter 4 provides a review of the American national system written from a legal perspective. Brian L. Nelson begins by highlighting corporate governance rules and requirements in the U.S., and it synthesizes rather painfully the current initiatives in the U.S. for the New York Stock Exchange and the provisions of the Sarbanes-Oxley Act of 2002. However, a more succinct and critical overall description of the requirements, perhaps in a tabular form, would have been more appropriate to the overall aims and objectives of the book.

Chapter 5 is contributed by David A. H. Brown and covers the Canadian national system of corporate governance. It highlights the principal players of corporate governance in Canada and briefly describes their roles in the current corporate governance systems. The following sections of the chapter examine the issues surrounding the performance of Canadian corporate governance practices and Canadian companies' corporate performance metrics on a number of key corporate governance issues. The chapter concludes that it is impossible to ascertain the international comparative standing of Canadian companies, because there is no reliable central source that compares governance activities and results of countries.

In Chapter 6, a similar approach to the Canadian corporate governance is adopted by Fabrice Bien, Jacques Délga, and Alain Ged, who examine the French national system. This chapter provides brief historical perspective of the French corporate governance system before providing overall legal sources of corporate governance publications. The rest of the chapter summarizes aspects of corporate governance practices in France, including: functioning of the boards; directors' rights and obligations; boards; audit and remuneration committees; and shareholder rights; as well as employees' considerations pertinent to corporate governance practices in France.

Chapter 7 is written by Masaru Yoshimori on the Japanese national system. Yoshimori suggests that the Japanese corporate governance system is converging on the U.S. model for increased oversight effectiveness, but he questions whether the optional U.S. style board structures will be adopted by the majority of Japanese firms.

The next two chapters, both contributed by Simon S. M. Ho, adopt a structure similar to the previous chapters and examine the corporate governance practices in Hong Kong and China. As for other countries, both of these chapters begin by noting historical changes in corporate governance systems. This is then followed by some typical legal and regulatory requirements of the corporate governance framework and ownership structures. Ho indicates that the Chinese government is still the largest shareholder and that there is also a high concentration of share ownership by the state or legal-person block holders in China. There is also a brief summary of executive compensation, corporate transparency, and disclosure quality in Chinese companies. The author concludes that many of the disclosure and accounting rules are too vague, as well as difficult to interpret or to enforce, and that this makes it difficult to obtain accurate accounting information from Chinese companies that are reluctant to accept fundamental changes in values.

Chapter 10 provides a summary of the recent changes in the Turkish national system of corporate governance. Melsa Ararat and Mehmet Ugur argue that economic reforms following the 2001 financial crisis in Turkey have resulted in improvements in corporate governance rules, not only via regulatory requirements, but also by voluntary improvements, as equity finance increasingly becomes a more popular alternative financing option among the major corporations. Despite these improvements in corporate governance rules, the ownership structures of Turkish companies remain highly concentrated, making it difficult to enforce further reforms. Results of a recent survey carried out among 52 Turkish companies indicated that Turkish companies have relatively good financial transparency, but the disclosures of board and management structures and processes are significantly worse than the rest of the world.

In Chapter 11, Raymon Leban and Tetyana Pasechnyk and, in Chapter 12, A. Naciri provide a summary of the transition economies' systems of corporate governance and those of the MENA countries, respectively. Transition economies are described as including those from the Commonwealth of Independent States (CIS) and the Central and Eastern European Countries. The MENA region refers to 18 countries, which include such economically diverse regions as oil-rich economies, e.g., the Gulf States, and relatively resource-scarce countries, e.g., Egypt, Tunisia, and Jordan. In both of these chapters the corporate governance framework is discussed, and examples of ownership patterns and financial reporting requirements, minority shareholder protection, and the responsibilities of boards of directors in some of the selected countries are noted. It is indicated that in all of these countries there are significant improvements in corporate governance rules, but family businesses are still the backbone of these countries' economies.

Chapter 13 considers corporate governance in the European Union. Naciri begins with an overview of the EU corporate governance framework, which is based on the three fundamental pillars: consensus, principles-based rather than binding rules, and comply and explain. Shareholder rights and their equitable treatment, disclosure and transparency, auditing practices, and the boards of directors and their committees and employee representations are briefly discussed in this chapter.

The book ends with some concluding remarks. Despite the fact that the most notable national corporate governance reforms throughout the world recognize the significant influence of the American vision of corporate governance, the main findings seem to be cautionary. Nations trying to improve their own corporate governance systems should consider their own socio-cultural environments and economic interests, and should develop systems of corporate governance that are in tune with their own needs. In this process, justice, transparency, honesty, and corporative democracy principles should not be ignored. This chapter then considers the implications of the

Sarbanes-Oxley Act on corporate governance national systems and speculates on the direction of future developments in corporate governance. Naciri concludes that corporate governance practices around the world will converge, and he warns that international regulatory institutions relevant to developing countries should be given opportunities to provide inputs into this process.

Of the 13 contributors, five are based in France and two in Canada. For a book claiming to give a perspective of governance "around the world," one of the main limitations is that it emphasizes the U.S. and Canadian systems of corporate governance and perhaps lacks an in-depth analysis of the systems in the major European countries. The treatment of all the European countries in one chapter may limit the appeal of the book to European markets, as the corporate governance systems in the EU are too divergent to be treated in a single chapter.

There is an overriding similarity in all of the contributions to the book: they are all very descriptive in nature and tend to summarize the recent legal and accounting changes relating to corporate governance practices in these countries. The book is otherwise well written and reasonably comprehensive. In order to make this book stand out from its competitors, more emphasis should have been placed on cultural and institutional perspectives unique to each country, and the editor should have included more countries than those currently presented. Currently, few books on corporate governance deal with theoretical perspectives, and I commend the editor for his courage and novelty in his approach to a topic that has so far attracted much attention to its key legal and institutional reforms. However, one of the limitations of the book is that it fails to draw on this conceptual framework in the subsequent country studies, where recent corporate governance changes are presented without discussing their conceptual underpinnings.

For the book to have more academic appeal, it should also integrate the theoretical issues raised at the outset with the subsequent chapters, and it should have more scope for case study material and self-assessment exercises. It could, however, be used as a supplementary text to main corporate governance textbooks. Unfortunately, the nature of the book is such that time is against it. With the introduction and adoption of new corporate governance reforms, practices around the globe are ever-changing, and the book is therefore rendered out of date very quickly. Despite these limitations, I would recommend it to any supervisor with students interested in corporate governance, and the book should certainly be held in the library of any university with undergraduate, postgraduate, or doctoral programs in accounting and finance.

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JOHN RICHARD EDWARDS and STEPHEN P. WALKER (editors), *The Routledge Companion to Accounting History* (London, U.K.: Routledge, 2009, ISBN 10:0-415-41094-0, pp. xvii, 619).

In the introduction to a book published 20 years ago—*A History of Financial Accounting*—J. R. Edwards felt it necessary to justify his work by offering a lengthy answer to the question "Why bother with accounting history?" Today, the two editors of this *Routledge Companion to Accounting History* have not experienced the same need. Is this because the advantages of understanding accounting history no longer need to be proven? It is perhaps too early to make such an assertion. But there is a growing interest in this discipline, perhaps simply out of sheer curiosity, or otherwise to support a scientific position on accounting, so much so that our readership includes some subscribers to *The Accounting Review*. I am tempted to say the following to these readers: "Irrespective of the specific subject of your research in accounting, there is little doubt that you will find something useful and relevant in this book."

This book is indeed the culmination of many years of university research in accounting history, not only published in a number of accounting journals, but also and especially in the three specialist journals in the field of accounting history: *The Accounting Historians Journal*, founded in 1977; *Accounting, Business and Financial History*, founded in 1990; and *Accounting History*, founded in 1995. Up to now, however, the dissemination of university research has been such that the resulting knowledge is dispersed among a number of scattered papers. To bring these fragments together in one work, therefore, is the aim of this hefty tome (619 pages). The production of such a large number of historical papers has meant that a significant number of scholars have become adept in the techniques of historical research. Because "professional" historians had little or no interest in accounting history, there developed a group of academics working in accounting faculties or departments who gradually became accounting historians, in the same manner as there are legal historians, business historians, or diplomatic historians. It is not therefore only the quantity of research that is remarkable, but also the considerable improvement in the quality of this work. Many amateur historians or people with a simple interest in the past have thus gradually become serious researchers, well versed in the methods used by professional historians and able to give meaning to the findings they extract from archives. While it was necessary to make this synthesis, owing to the wide scattering of the individual works on the subject, it was made possible by the increased number of authors qualified as historians. As Christopher Napier puts it: "At the beginning of the twenty-first century, historical accounting research has certainly reached a stage of maturity ..." (p. 39). This book has therefore arrived just at the right time to consolidate the knowledge of this "free-standing international community."

The book therefore aims to provide a synthesis of our knowledge on accounting history, in the form of 28 papers (by 37 co-authors), edited by two seasoned fellow accounting historians who are particularly well placed to observe what has been going on in the field of accounting history during the last 30 years. Apart from the many papers and books published by them, John Richard Edwards and Stephen P. Walker are the editor and the former editor, respectively, of two refereed journals in the field of accounting history, and they are members of one of the most active Business History Research Units (at the University of Cardiff, U.K.).

The high literary standard of the writings makes them easy and, on the whole, pleasant to read. In addition, there is a convenient list of "key works" (i.e., outstanding articles or books) at the end of each article. It does not, however, present accounting history from its beginnings to the present day, but is theme-based, allowing readers to dip into each topic as they like, according to their individual spheres of interest. The special focus on emerging subjects (p. 4) offers yet another advantage to researchers.

Each of the 28 articles, grouped under seven different sections,[1] deals with the history of accounting from a particular angle, such as, for example, taxation, railroads, education, professionalization, financial accounting theory, management accounting theory and practice, capitalism, national accounting, religion, and the State. Structuring the book in this way is particularly useful for accounting researchers who are looking for information and insights into a particular subject. It also avoids the double pitfall of holism and over-dispersion: On the one hand, the *Routledge Companion* does not aspire to a "lofty form of holism" (p. 3), and on the other hand, it begins the task of relating parts to a whole.

The first part, entitled "The Discipline," provides an outline of accounting history, considered as a sub-discipline organized around a particular scientific community with its own associations and meeting places. Non-specialists will not linger in these first three chapters, but anyone familiar with accounting history will delight in the astute and highly pertinent analyses presented regarding the evolution of a scientific territory and community. It is not a coincidence that the two co-editors of the book wrote two of the three contributions. The article by Christopher Napier presents the historiography of the discipline, with an emphasis on the trends of thought that developed therein. He shows the gradual separation of two visions of accounting history. The first, which he calls the "history of accounting," concerns accounting techniques in particular. "Here, the researcher's objective is to understand accounting as a set of procedures or practices" (p. 31). The other category is "socio-historical accounting research" in which researchers explore the way in which individuals and organizations interact in accounting.

One of the greatest concerns of the community of accounting historians is how to give their research a truly international dimension: on the one hand, researchers are mainly interested in the history of their own country, and on the other, the literature read internationally amounts to what is published in English. As a result, "there are concerns about the Anglo-Saxon dominance of the discipline. Most authors in English-language journals tend to focus on the accounting histories of their own countries post-1900" (p. 65). Furthermore, all attempts to develop an international and comparative history have tended to be in vain, and the fact remains "that collaboration, particularly international alliances, is less common in accounting history research" (p. 63) than in the general accounting literature. Under these conditions, it is difficult to avoid the historiography presented to us as being, as historians have often remarked, simply the "historiography of the winners," that is to say, the historiography of those whose native language has become the language of international communication for scholars. Unfortunately, the cost of translation is high and does not allow all the documentation to be available in even one other language, and we cannot expect researchers to speak four or five different languages. We have to admit that this *Routledge Companion to Accounting History* gives us little insight into the history of accounting systems in continental Europe and Japan.

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[1] The Discipline, Technologies, Theory and Practice, Institutions, Economy, Society and Culture, and Polity.

Part 2 is dedicated to "Technologies." This title actually covers a wide diversity of work, ranging from "ancient accounting" (mainly in Mesopotamia and ancient Egypt) and "bookkeeping" (from Greece and Rome to the late 19th century) to "mechanisation and computerisation" in the 20th century.

Part 3 deals with "Theory and Practice." Four chapters are necessary to cover the entire field of what we call accounting: "financial accounting theory," "financial accounting practice," "management accounting theory and practice," and "auditing." The three chapters about financial accounting and auditing will no doubt be of greatest interest to the readership of *The Accounting Review*. The first "provides a history of financial accounting theory in the English-speaking world" (p. 139), and the other two are in a similar vein. It is of some regret that these chapters focus on the period after 1870 and on the Anglo-Saxon world. The recent difficulties encountered in the harmonization of international accounting revealed the existence of several viewpoints (German, Japanese, and especially Latin) that cannot be easily reconciled and harmonized. These viewpoints and their harmonization would have deserved more attention from scientific researchers, especially as there is a certain amount of literature on the subject to which researchers could have been asked to contribute. Written by one of the leading scholars in accounting history, the chapter concerning management accounting adopts a much more international approach and considers both the "Iberian roots" and "French cost accounting theory" as well as "recent costing developments in Asia." The lengthy bibliography at the end of this article testifies to the erudition of the author and his openness to the rest of the world.

Part 4 is dedicated to "Institutions." The history of professionalization is first presented, with an account of how accountants are trained in different countries. Some highly interesting questions are raised, when the author mentions the great differences between "the Anglophone model of professionalism" and what has happened in France, Belgium, Spain, and Portugal or in the "former planned economies." A second chapter is devoted to the history of "multinational professional service firms (the Big Four)" in the last 30 years, a third looks at the history of training accountants (accounting education), and a fourth deals with "accounting regulation." In this last chapter, we have some interesting ideas on "the interplay between general theories of regulation and histories of accounting regulation" (p. 317). The emphasis is, however, placed on the impact of the U.S. Securities Acts of 1933/34, and regulation processes outside the Anglo-Saxon world are not considered.

Part 5 is entitled "Economy" and comprises five chapters: "Capitalism," "National Accounting," "Finance and Financial Institutions," "Railroads," and "Scandals." Those who wish to learn more concerning the discussions about the link between double-entry bookkeeping and capitalism will find in the first article a summary of the debate that ran through the entire 20th century. The theme of "national accounting" is given little attention by accounting historians owing to the fact that it concerns economics and statistics more than actual accounting in the most common meaning of the term. The wealth of information and variety of documentation collected for this article give it especial interest, however. We learn in particular that national accounting is a system of accounts whose standardization was entrusted to the United Nations. The other three articles deal with subjects already widely covered outside the circles of accounting historians.

Part 6 is devoted to "Society and Culture." It deals with themes that have only recently appeared in the field of accounting history. Bridges have been built between accounting history and some apparently far-removed themes such as gender, race and ethnicity, indigenous people and colonialism, emancipation, religion, and creative arts. Subjects studied include the position of women and "non-whites" in the accounting profession, the position of accounting in the colonization process or in religious organizations, the influence of religious thinking on accounting practices, and the position of accounting in the creative arts.

The last part deals with "Polity" and tackles three issues: "the State," "military," and "taxation." In the first, accounting is viewed "as part of the accountability mechanisms of states" (p. 543), from the heyday of antiquity to the present day. The second examines military themes in accounting history as well as accounting for military performance. The third explores tax issues and shows how the history of taxation, already well documented, can shed light on accounting history.

The 37 authors and joint authors who wrote the articles were instructed "to provide a balanced overview of current knowledge, identify issues, discuss relevant debates, and reflect on research opportunities" (p. 3). We can consider, after reading the book, that they have fulfilled their mission well. As for the editors, they have managed to organize the publication of a coherent and captivating book, a mammoth task in a field characterized by dispersion and disorder.

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CAPSULE COMMENTARY

By the Book Review Editor

NERYS BAILEY (editor), *Harold Cecil Edey: 20th Century Accounting Reformer, An Autobiography* (ISBN 978-0-9561219-0-5; pp. xv, 162; copies obtainable by writing Nerys Bailey at donald.bailey1@virgin.net; £15 in U.K., £18 elsewhere).

Harold C. Edey spent his entire academic career at the London School of Economics (LSE), from which he retired in 1980. He published many of his articles and other papers in *Accounting Queries* (Garland Publishing, 1982), and a book of essays in his honor, *External Financial Reporting*, edited by Bryan Carsberg and Susan Dev, was published by Prentice/Hall International in cooperation with the LSE in 1984. He played important roles in the Institute of Chartered Accountants in England and Wales. He became the first full-time accounting academic to serve on an Institute committee, in 1964, and to be named to the Institute's Council, in 1969. He was one of the founding members of the Institute's Accounting Standards Steering Committee in 1970, serving for 12 years. In the 1950s, he was among the leading figures of the academic body known today as the British Accounting Association.

Edey also held important administrative positions in both the LSE and the University of London, and in the 1970s he was an influential adviser to emerging accounting departments in other U.K. universities on appointments to chairs.

This engaging autobiography gently chronicles Edey's personal life and professional career from his birth on February 23, 1913 until his death on March 12, 2007, accompanied by many pictures of family, colleagues, and important certificates as well as six informative appendices. The editor of the autobiography and compiler of the volume is Edey's daughter, Nerys Bailey.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and editors are responsible for providing constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary.*

FORMAT

1. All manuscripts should be formatted in 12-point font on 8 1/2 x 11" paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years.* All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a cross-sectional equation; re-form.* See *Webster's* for correct usage.

Keywords: The abstract must be followed by at least three keywords to assist in indexing the paper and identifying qualified reviewers.

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ABSTRACT/INTRODUCTION

An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

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3. For cited works that include more than one work by an author (or same co-authors) that are published in the same year, the suffix a, b, etc., is to follow the date in the within-text citations and in the "References" section. For example: (Baiman and Rajan 2002a, 2002b).
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2. Arrange citations in *chronological order* when two or more works are by the same first author (regardless of co-authors). Two or more works by the *same author(s)* published in the same year are distinguished by letters a, b, etc., after the year.
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Sample entries are as follows:

Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.

———, and ———. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.

Berry, R. 2003. Testimony before the Senate Committee on Homeland Security and Governmental Affairs Permanent Subcommittee on Investigations. November 18. Available at: http://hsgac.senate.gov/_files/111803berry.pdf.

Cohen, D., A. Dey, and T. Lys. 2005. The Sarbanes Oxley Act of 2002: Implications for compensation structure and risk-taking incentives of CEOs. Working paper, New York University, University of Chicago, and Northwestern University.

Cole, R., and T. Yakushiji, eds. 1984. *The American and Japanese Auto Industries in Transition.* Ann Arbor, MI: University of Michigan.

Dechow, P. M., R. Sloan, and A. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.

———, S. P. Kothari, and R. L. Watts. 1998. The relation between earnings and cash flows. *Journal of Accounting and Economics* 25: 133–168.

———, and I. Dichev. 2002. The quality of accruals and earnings: The role of accrual estimation errors, *The Accounting Review* 77 (Supplement): 35–59.

Easton, P. 2003. Discussion of: The predictive value of expenses excluded from pro forma earnings. *Review of Accounting Studies* 8: 175–183.

Engel, E., R. Hayes, and X. Wang. 2007. The Sarbanes-Oxley Act and firms' going-private decisions. *Journal of Accounting and Economics* (forthcoming).

Financial Accounting Standards Board (FASB). 2006. *Accounting for Uncertainty in Income Taxes, and Interpretation of FASB Statement No. 109.* FASB Interpretation No. 48. Financial Accounting Series. Norwalk, CT: FASB.

Fehr, E., and K. Schmidt. 2003. A theory of fairness, competition, and cooperation. In *Advances in Behavioral Economics*, edited by C. Camerer, G. Loewenstein, and M. Rabin, 271–296. New York, NY: Princeton University Press.

Janis, I. L. 1982. *Groupthink: Psychological Studies of Policy Decisions and Fiascoes.* Boston, MA: Houghton Mifflin.

Levitt, A. 1998. The numbers game. Speech delivered at New York University, Center for Law and Business, September 28.

Maggi, G. 1999. The value of commitment with imperfect observability and private information. *RAND Journal of Economics* (Winter) 30: 555–574.

National Commission on Fraudulent Reporting (the Treadway Commission). 1987. *Report of the National Commission on Fraudulent Financial Reporting.* Washington, D.C.: NCFFR.

Nelson, M. W. 2003. Behavioral evidence on the effects of principles- and rules-based standards. *Accounting Horizons* 17 (1): 91–104.

———. 2005. A review of experimental and archival conflicts-of-interest research in auditing. In *Conflicts of Interest: Challenges and Solutions in Business, Law, Medicine, and Public Policy,* edited by D. A. Moore, D. M. Cain, G. Loewenstein, and M. H. Bazerman. Cambridge, U.K.: Cambridge University Press.

Rigdon, E. E., R. E. Schumacker, and W. Wothke. 1998. A comparative review of interaction and nonlinear modeling. In *Interaction and Nonlinear Effects in Structural Equation Modeling*, edited by R. E. Schumacker, and G. A. Marcoulides, 1–16. Mahwah, NJ: Erlbaum Associates.

Schultz, E., and T. Francis. 2002. Companies profit on workers' deaths through "dead peasants" insurance. *Wall Street Journal* (April 19): 1.

Securities and Exchange Commission (SEC). 2002. *Certification of Disclosure in Companies' Quarterly and Annual Reports.* Release Nos. 33-8124, 34-46427. Washington, D.C.: SEC.

U.S. House of Representatives. 2002. The Sarbanes-Oxley Act of 2002. Public Law 107-204 [H. R. 3763]. Washington, D.C.: Government Printing Office.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be inserted using the "footnote" or "endnote" feature of the word processing software which will automatically number the footnotes throughout the manuscript with superscript Arabic numerals.

SUBMISSION OF MANUSCRIPTS

Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state upon submission that the work is not submitted or published elsewhere.
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4. A submission fee is required unless explicitly waived by the editor for invited revisions of previous submissions. The submission fee should be paid online (VISA or MasterCard only) at: https://aaahq.org/AAAforms/journals/tarsubmit.cfm. If unable to pay online, a check in U.S. funds, made payable to the American Accounting Association, may be sent to Ms. Mary Capps, Editorial Assistant, *The Accounting Review*, The University of Texas at Austin, McCombs School of Business, Department of Accounting CBA 4M.202, 1 University Station B6400, Austin, TX 78712-0211. The submission fee in U.S. funds is $200.00 for members and $400.00 for nonmembers of the AAA. The submission fee is nonrefundable.
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The Accounting Review welcomes submissions of comments on previous *TAR* articles. Comments on articles previously published in *The Accounting Review* will generally be reviewed by two reviewers, usually including an author of the original article (to assist the editor in evaluating whether the submitted comment represents the prior article accurately) and an additional reviewer who is independent of the original article. If a comment is accepted for publication, the original author will generally be invited to reply. All other editorial requirements, as enumerated above, apply to proposed comments.

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The deadline for free position ads to be included in this section is two months prior to the date of publication. *The Accounting Review* is published in January, March, May, July, September, and November. Position ads in the "Placement Ads" are free with the purchase of a job posting in the AAA Career Center. For more information on how to purchase a job posting in the Career Center and receive your free position ad in *The Accounting Review*, go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

401 ALFAISAL UNIVERSITY, College of Business, Riyadh, Saudi Arabia, is seeking highly qualified accounting faculty for Fall Term 2009. An earned Ph.D. from a nationally recognized university is preferred for this position. ABDs and M.ACC.s with a CPA will be considered. Applicants must provide evidence of teaching in the position area, research potential in business, and excellent communications skills. We particularly solicit applicants who seek to conduct significant research. Salary exceeds AACSB levels and is based on qualifications. Load is currently four courses per year. Benefits include free housing and utilities, insurance, education subsidy, and summer leave airfare for candidate and family. Additional information can be found at our website: http://www.alfaisal.edu. Applications must include a cover letter describing qualifications and research interests, *vitae*, and the names, addresses, and telephone numbers of three references. Please email applications to Dean Norman Wright at nwright@alfaisal.edu. Applications will be accepted until the position is filled.

402 UNIVERSITY OF NEVADA, RENO, Department of Accounting and Information Systems is seeking candidates for one full-time, tenure-track, Assistant Professor in Accounting. Candidates with interests in teaching financial accounting are especially sought, but all specialties are encouraged to apply. Duties of the position include teaching at both the undergraduate and graduate (master's degree) levels, making intellectual contributions, and participating in service activities within the university, academic, and professional communities. For consideration, apply online at: http://www.unrsearch.com/applicants/Central?quickFind=53782. Dr. Jeff Wong and members of the search committee will be attending the AAA Annual Meeting in New York, August 2009. All interested candidates are encouraged to schedule a meeting to learn more about this accounting position at the University of Nevada, Reno. Women and under-represented groups are encouraged to apply. EEO/AA.

403 FLORIDA ATLANTIC UNIVERSITY, School of Accounting invites applications for tenure-track faculty positions at the rank of Assistant Professor starting August 2010, subject to budgetary approval (positions #990458, #991404, and #992952). The Barry Kaye College of Business is accredited by the AACSB. Preference will be given to those qualified in the Accounting Systems and Financial Accounting areas. Candidates must have earned a Ph.D., or be close to completing the degree, and exhibit strong potential for and commitment to excellence in research and teaching. Salary is competitive. To be considered for these positions, applicants must complete the online Faculty, Administrative, Managerial & Professional Position application through the Office of Human Resources at: https://jobs.fau.edu. Please include a letter of application, *curriculum vitae*, names of three references, and a sample of your research with the online application. FAU faculty will be attending the AAA Annual Meeting in New York, August 2009, and will be available to meet with interested candidates. For additional information, please email Dr. Mark Kohlbeck, chair of the recruiting committee, at: soa@fau.edu. A background check will be required for the candidates selected for these positions. Florida Atlantic University is an Equal Opportunity/Equal Access Institution.

404 WASHINGTON & LEE UNIVERSITY, Department of Accounting, invites applications for a tenure-track position at the Assistant Professor level, beginning Fall, 2010. Teaching responsibilities will include upper-level courses in financial accounting and possibly auditing, along with introductory courses. Applicants should have a doctorate from an AACSB-accredited institution, or be close to completion. Evidence or promise of high-quality teaching and scholarship is expected. Washington & Lee is a small, highly selective, private university, located in the scenic Shenandoah Valley. The Williams School of Commerce, Economics, and Politics has been AACSB-accredited since 1927. Interested parties should send a cover letter, *curriculum vitae*, evidence of teaching effectiveness, and research papers to Bill King, Department of Accounting, Washington & Lee University, Lexington, VA 24450; Email: kingw@wlu.edu. Review of applications will begin immediately and will continue until the position is filled. Washington & Lee is an Equal Opportunity/Affirmative Action Employer.

405 LOYOLA MARYMOUNT UNIVERSITY, Department of Accounting invites applicants for two full-time tenure-track faculty positions at Assistant/Associate rank effective Fall 2010. A doctorate (or short-term ABD status) in Accounting from an AACSB-accredited university is required. All candidates should demonstrate a strong commitment to scholarly research and excellence in teaching. All areas of accounting teaching will be considered. Salary is competitive and research support is available. If you are interested in learning more about these positions, contact: Professor Mahmoud Nourayi; Email: mnourayi@lmu.edu; or Phone: (310) 338-5831. Loyola Marymount, a comprehensive university in the mainstream of American Catholic higher education, seeks professionally outstanding applicants who value its mission and share its commitment to academic excellence, the education of the whole person, and the building of a just society. Visit http://www.lmu.edu for more information. LMU is an Equal Opportunity institution actively working to promote an intercultural learning community. Women and minorities are encouraged to apply.

406 WILLIAM PATERSON UNIVERSITY, Cotsakos College of Business, Department of Accounting and Law, accredited by AACSB, invites applications for a tenure-track position at the Assistant/Associate rank beginning Fall 2009. An earned doctorate in accounting (ABD with evidence that completion is certain within one year) from an AACSB-accredited institution; evidence of successful teaching in accounting; and an established record or promise of research and significant achievement in scholarly activity are required. Expertise in auditing is preferred. Ability to teach accounting courses at both the undergraduate and graduate levels and to develop and design courses that integrate the use of a state-of-the-art Financial Learning Center and Trading Room into the Accounting and Law curriculum is required. Send a letter of interest, including Job Code 908CHE, *curriculum vitae*, and three letters of recommendation to: Dr. Sia Nassiripour, Chairperson, Department of Accounting and Law, Cotsakos College of Business, William Paterson University, 1600 Valley Road, Wayne, NJ 07470. Review of applications will commence immediately and continue until the position is filled. For more information visit: http://www.wpunj.edu. William Paterson University is an Equal Opportunity Employer committed to diversity. Minority and women candidates are encouraged to apply.

407 SOGANG UNIVERSITY, School of Business, Accounting Department, invites applications for tenure-track positions at the Assistant/Associate Professor level starting September 2009 or later; rank dependent on qualifications and experience consistent with University policy. The candidate should have or complete a doctoral degree in Accounting before the academic appointment begins. The candidate should demonstrate potential for success in scholarly research and excellence in teaching. Qualified individuals in all specialty areas of accounting will be considered but candidates with interests in Financial or Managerial Accounting are preferred. Review of applications will begin immediately and continue until the positions are filled. Candidates are requested to submit their *vitae* and contacts for three references by email to: Kookjae Hwang, Accounting Search Chair, School of Business, Sogang University, Seoul, Korea, 121-742; Email: khwang@sogang.ac.kr.

408 FLORIDA STATE UNIVERSITY, Department of Accounting, invites applications for a tenure-track position. Employment to begin the Fall 2009 or Spring 2010. Candidates must be interested in teaching auditing and/or forensic accounting. Candidates must have a Ph.D. in accounting and experienced applicants should have a well-established record of publications in high-quality journals and of effective teaching. Expectations include commitment to excellence in research, teaching, and service, including working with Ph.D. students. Applicants should send their *vita* to: Bud Fennema, College of Business, Florida State University, PO Box 32306-1110, Tallahassee, FL 32306-1110; or Email: fennema@fsu.edu.

409 MISSISSIPPI STATE UNIVERSITY, College of Business and Industry, Adkerson School of Accountancy, invites applicants to apply for a Fall 2009 Endowed Chair (Full Professor), Endowed Professorship (Associate or Full Professor), or Assistant Professor nine-month tenure-track position to assist with leadership and involvement in the School's doctoral program. Any area of specialization will be considered. Candidates must have an accounting doctorate degree, demonstrated excellence in the field of accountancy and experience in working with doctoral students for Full and Associate Professor. ABD candidates will be considered. Professional certification and accounting experience are highly desirable. Level of appointment commensurate with qualifications and experience. Mississippi State University, located in Starkville, MS, offers a unique university town setting with a great environment to live and work. Candidates must complete the Personal Data Information Form online. Send letter of interest, current *curriculum vitae*, and names (including contact information) of three professional references to: Chair, Search Committee, Adkerson School of Accountancy, PO Box EF, Mississippi State, MS 39762; or Email Louis Dawkins, Director at: led23@msstate.edu. Position will remain open until filled. MSU is an AA/EEE.

410 AUBURN UNIVERSITY MONTGOMERY, Department of Accounting and Finance, invites applicants for a tenure-track accounting position at the Assistant, Associate or Full Professor level beginning August 2009. Candidates must hold a doctoral degree or be ABD and near completion. Applicants must have demonstrated potential for quality teaching and research. While qualified individuals in any specialty area of accounting will be considered, the primary need is in financial accounting. Auburn University Montgomery is the metropolitan campus of the state's land grant university. The School of Business is accredited by the AACSB at both the graduate and undergraduate levels. Review of applications will continue until the position is filled. Contact: Dr. Lee Gurley, Head, Department of Accounting and Finance, School of Business, Auburn University Montgomery, PO Box 244023, Montgomery, AL 36124-4023; Phone: (334) 244-3227; Email: lgurley@.aum.edu. Auburn University Montgomery is an EEO/AA Employer and Educator.

411 CENTRAL COLLEGE invites applications for an Assistant Professor of accounting. This is a full-time, tenure-track appointment beginning August 2009. Minimum qualifications include a M.B.A./M.A. in accounting with CPA/CMA designations. Candidates with college teaching experience and /or holding a Ph.D. in accounting will be given preference. The successful candidate will be expected to teach an auditing course and other courses among the following: introductory financial and managerial accounting, intermediate accounting, advanced financial accounting, tax accounting, accounting information systems, or advanced managerial accounting. The normal teaching load is 21–23 semester credit hours per academic year. Visit Central's website at: http://www.central.edu/jobseekers for a full position description and application process. AA/EOE.

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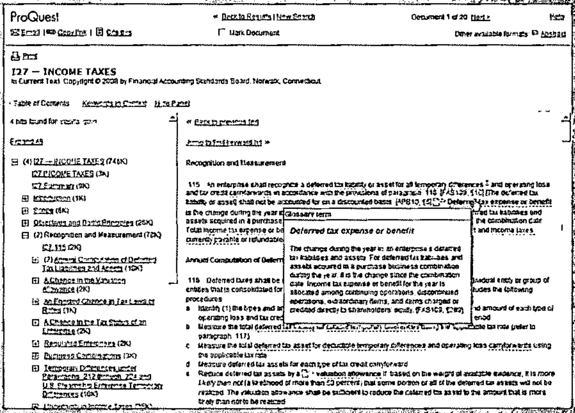
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